PHILOSOPHICAL

TRANSACTIONS

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MDCCCXXX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion,

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as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the ROYAL MEDALS for the year 1829 as follows:

A Royal Medal to CHARLES BELL, Esq. Fellow of the Royal Society, for his Discoveries relating to the Nervous System.

A Royal Medal to Professor EILERT MITSCHERLICH of Berlin, Foreign Member of the Royal Society, for his Discoveries relating to the Laws of Crystallization, and the Properties of Crystals.

PHILOSOPHICAL TRANSACTIONS.

I. The Bakerian Lecture.—*On the manufacture of Glass for optical purposes.*

By Michael Faraday, *Esq. F.R.S. &c.*

Read November 19, December 3 and 10, 1829.

Introduction.

PERFECT as is the manufacture of glass for all ordinary purposes, and extensive the scale upon which its production is carried on, yet there is scarcely any artificial substance in which it is so difficult to unite what is required to satisfy the wants of science. Its general transparency, hardness, unchangeable nature, and varied refractive and dispersive powers, render glass a most important agent in the hands of the philosopher engaged in investigating the nature and properties of light; but when he desires to apply it, according to the laws he has discovered, in the construction of perfect instruments, and especially of the achromatic telescope, it is found liable to certain imperfections, not essentially existing, but almost always involved during its preparation, and fatal to its use. These are so important and so difficult to avoid, that science is frequently stopped in her progress by them; a fact fully proved by the circumstance that Mr. Dollond, one of our first opticians, has not been able to obtain a disc of flint glass four inches and a half in diameter, fit for a telescope, within the last five years, or a similar disc of five inches in diameter within the last ten years.

It must be well known to the scientific world, that these difficulties have induced some persons to labour hard and earnestly for years together, in hopes of surmounting them. Guinand was one of these: his means were small, but he deserves the more honour for his perseverance and his success. He commenced the investigation about the year 1784, and died engaged in it in the

year 1823. Fraunhofer laboured hard at the solution of the same practical problem. He was a man of profound science, and had all the advantages arising from extensive means and information, both in himself and others. He laboured in the glass-house, the work-shop, and the study, pursuing without deviation the great object he had in view, until science was deprived of him also by death. Both these men, according to the best evidence we can obtain, have produced and left some perfect glass in large pieces: but whether it is that the knowledge they acquired was altogether practical and personal, a matter of minute experience, and not of a nature to be communicated; or whether other circumstances were connected with it,—it is certain that the public are not in possession of any instruction, relative to the method of making a homogeneous glass fit for optical purposes, beyond what was possessed before their time; and in this country it seems doubtful whether they ever attained a method of making such glass with certainty and at pleasure, or have left any satisfactory instructions on the subject behind them.

The philosophical deficiencies referred to above, induced the President and Council of the Royal Society in 1824, to appoint a Committee for the improvement of glass for optical purposes, consisting of Fellows of the Royal Society and Members of the then Board of Longitude. The Government on being applied to, not only removed the restrictions to experiments on glass, occasioned by the Excise laws and regulations, but undertook to bear all the expenses of furnaces, materials, and labour, as long as the investigations offered a reasonable hope of success. In consequence of these facilities, a small glass-furnace was erected in 1825, and many experiments both upon a large and small scale were made with flint and other glasses. During their continuance, Messrs. Green and Pellatt gave every instruction and assistance in their power, and evinced the most earnest desire for success. The researches, however, soon showed themselves to be a work of labour, which, to be successful, would require to be pursued unremittingly for a long period; and on May 5, 1825, a sub-committee was appointed, to whom the direct superintendence and performance of experiments were entrusted. This committee consisted of Mr. Herschel, Mr. Dollond, and myself; but in March 1829 was reduced to two, by the retirement of Mr. Herschel, who about that period went to the continent. From the respective pursuits of the three persons appointed upon this committee it may be easily

gathered, that though all were to do what they could in every way for the general good of the cause in which they were jointly engaged, yet a distinction in the duties of each existed. It was my business to investigate particularly the chemical part of the inquiry; Mr. Dollond was to work and try the glass, and ascertain practically its good or bad qualities; whilst Mr. Herschel was to examine its physical properties, reason respecting their influence and utility, and make his competent mind bear upon every part of the inquiry.

The experimental glass-house was erected on a part of the premises of Messrs. Green and Pellatt, at the Falcon Glass-works; whilst my duties as Director of the Laboratory of the Royal Institution, required my presence almost constantly at the latter place, nearly three miles from the former. As I found it impossible under these circumstances to make the numerous experiments and pay that close attention which appeared essentially necessary to produce any degree of success, the President and Council of the Royal Society applied to the President and Managers of the Royal Institution, for leave to erect on their premises an experimental room, with a furnace, for the purpose of continuing the investigation. They were guided in this by the desire which the Royal Institution has always evinced to assist in the advancement of science; and the readiness with which the application was granted, showed that no mistaken notion had been formed in this respect. As a member of both bodies, I felt much anxiety that the investigation should be successful. A room and furnaces were built at the Royal Institution in September 1827, and an assistant was engaged, Sergeant Anderson of the Royal Artillery, whose steady and intelligent care has been of the greatest service to me in the experiments that have been proceeding constantly from that time to the present. At first, the inquiry was pursued principally as related to flint and crown glass; but in September 1828 it was directed exclusively to the preparation and perfection of peculiar heavy and fusible glasses, from which time to the present continual progress has been made.

I have thought it right to give this brief explanatory statement of the manner in which it has happened to become my duty, on the present occasion, to give an account of what has been done in the improvement of glass for optical purposes by the Committee of the Royal Society, working at the Royal Institution. I would willingly have deferred this account until the inquiry were more com-

plete than at present; for though glass has been made, and telescopes manufactured, yet I have no doubt that much more of improvement will be effected. It may be said that a long time has elapsed since the experiments were first instituted; and that if any thing could be done, it should have been effected in so long a period. But be it remembered, that it is not a mere analysis, or even the developement of philosophical reasoning, that is required: it is the solution of difficulties, which, as in the cases of Guinand and Fraunhofer, required many years of a practical life to effect, if it was ever effected. It is the foundation and developement of a manufacturing process, not in principle only, but through all the difficulties of practice, until it is competent to give constant success: and I may be allowed to plead the acknowledged difficulty and importance of the subject as a reason, both why it may not yet have obtained perfection, and why it should still be pursued.

My wish, however, to delay the account of the researches until I could have carried the experiments further, is overcome by the conviction that much more time must be expected to elapse before I shall consider the investigation finished; by the consideration that a decided step has been made in the manufacture of glass for optical purposes; and by the feeling that the Royal Society which instituted, and the Government which defrays the expenses of the experiments, have a right to an official account of the present state of the investigation. Although much useful information has been obtained respecting flint and other glasses, yet as that train of research is very imperfect, uncertain, and will probably be resumed, I shall confine my present statement altogether to the heavy optical glass already referred to. It will be impossible for me to describe all that has been done on this subject; but I shall endeavour to give such an account of the glass, and the process by which it is obtained in a homogeneous state, as shall enable other persons to do what has been done at the Royal Institution, without incurring the laborious prefatory experiments and investigations which we have had to undertake; only introducing so much of the latter, and the principles of the process, as are necessary to make the descriptions clear to a practical man, and enable him to avoid those circumstances which might otherwise occasion failure. That the paper may appear long and tedious I am aware; but it should be remembered, that it can have no other utility than as containing efficient instructions to the few who may desire to manufacture optical glass; and that

to render whatever of this character it may have, imperfect, for the sake of giving to it a more abbreviated and popular form, would have been doing injustice to the objects and motives of those who have instituted and supported the experiments.

§ 1. *Process of Manufacture, &c. &c.*

1. The general properties of transparency, hardness, and a certain degree of refractive and dispersive power, which render glass so valuable as an optical agent, are easily obtained: but there is one condition essential in all delicate cases of its application, which is not so readily fulfilled; this is, a perfectly homogeneous composition and structure. Although every part of the glass may in itself be as good as possible, yet without this condition they do not act in uniformity with each other; the rays of light are deflected from the course which they ought to pursue, and the piece of glass becomes useless. The streaks, striæ, veins or tails, which are seen within glass otherwise perfectly good, result from a want of this equality; they are visible only because they bend the rays of light which pass through them from their rectilinear course, and are constituted of a glass having either a greater or a smaller refractive power than the neighbouring parts.

2. When these irregularities are so powerful as to render their effects observable by the naked eye, it may easily be supposed to what an injurious extent their influence must extend in the construction of telescopes and other instruments of a similar nature, where these faults are not only magnified many times, but where the effect is to give an equally magnified erroneous representation of the object looked at, when the very point to be attained is to examine that object with the utmost accuracy; and it is accordingly found that these striæ are the most fatal faults of glass intended for optical purposes. Besides this, not only do the striæ themselves occasion harm, but there is every reason to believe that they rarely occur in glass otherwise homogeneous. Sometimes, it is true, a grain of sand, in passing through and at the same time dissolving in glass, will give a streak of different composition to the rest of the substance; and at others, a bubble ascending may lift a line of heavy or more refractive matter into a lighter and less refractive portion above. But very often, and especially as glass is usually manufactured and collected for use,

striæ are merely the lines or planes where two different kinds of glass approximate; and even if the striæ could be covered so as to produce no bad effect, yet the other parts, not being in every respect alike, would exert an unequal action on light, and the piece be therefore improper for the construction of a telescope. Many a disc, which upon the most careful examination has appeared perfectly free from striæ and quite uniform, has, when worked into an object-glass, been found incapable of giving a good image, on account of the existence of irregularities in the mass, which, though not sudden or strong enough to occasion striæ, still produce a confused effect; and if this happens with glass approaching so near to perfection, it happens still more frequently and to a much stronger degree with such as contain visible irregularities.

3. It must not be imagined that striæ, or those fainter differences, are, according to an expression sometimes used, due to impurity. The glass, either of the streak or of the neighbouring parts, would be equally good for optical purposes were it all alike. It is the irregularity that constitutes the fault; and hence, in this respect, a particular composition is of very little importance. As glass is always the result of a mixture of materials having different refractive and dispersive powers, it is evident that striæ must exist at one period during its preparation; and the point required is not so much to seek for a difference of composition, or for those proportions which are found by analysis to exist in specimens of tried and acknowledged good glass; as to devise and perfect a process by which the striæ period should be passed over before the glass is finished, and the formation of fresh striæ be prevented.

4. Besides these, there are other faults in glass. Sometimes it is said to be wavy, when it has the appearance of waves within its mass; but this is only a variety of that irregularity which has just been explained as constituting, when in a stronger degree, streaks and striæ. Occasionally appearances are observed in it, which seem to indicate a peculiar structure or crystallization, or an irregular tension of its parts: these, there is every reason to believe, may be avoided by careful annealing. Again: the glass sometimes includes bubbles, which, when small and numerous, render it what is called seedy. Bubbles are not usually considered as of much consequence to the performance of the glass, but objectionable only because of their appearance when the glass is looked at, rather than when looked through. They each act like a very

powerful but very small double convex lens of a rare substance in a very dense medium, or as equally deep double concave lenses of glass would do in air; they rapidly, therefore, turn the rays impingeing on them on one side, and occasion a loss of light, just as so many opaque spots would do. But as even when numerous their united area may amount to only a very small proportion of the area of the plate of glass required for a telescope, this loss of light is usually of but little consequence. In practice, it is said that no other real evil than such loss of light is dependent on them.

5. Of all these faults, that of the irregularity constituting streaks, striæ, and waves, is the most difficult to avoid, and the most injurious in its effect. It is not an improvement only beyond what is ordinarily done in this respect that is required, but absolute perfection, a homogeneity equal to that of pure water. In the two kinds of glass required to render a telescope achromatic, namely, crown or plate glass, and flint glass, it is the latter which is obtained perfect with the greatest difficulty, and to which therefore the greatest attention has been paid. The reason of this will be evident, if the general composition of the two glasses be taken into account. The required difference between them in refractive and dispersive power is found to be at command, by attention to composition; and it has been also ascertained, that crown and plate glass answer exceedingly well for the one variety, and flint glass for the other. Crown glass consists of silica, lime, oxide of iron, sometimes a little alkali, and small quantities of other matters: these substances are not very different in their refractive powers, and when fused do not produce very strong streaks, even though a little difference in the composition of different parts of the glass may exist. The glass also is not a very powerful fluxing agent upon the crucible in which it is melted; so that although it is in contact with it in a fluid and heated state for many hours, it does not dissolve much from it; and what it does dissolve having a refractive power little different from that of the glass itself, proportionately less harm is occasioned. Again: the specific gravity of the different materials used is not very different; so that the mixing agencies which affect the contents of the pot,—such as the ascent of bubbles, the ascending and descending currents from difference of temperature,—are more energetically exerted, and the whole mass approaches nearer to uniformity in a given time, or acquires it sooner than would happen were greater differences to exist.

6. With plate glass the same circumstances hold nearly in an equal degree. This substance is composed of silica and alkali essentially, other elements being only in small quantities. Its action upon the crucible is greater than crown glass, but then it has a second application of heat in such circumstances as are calculated to give a very uniform temperature to the contents of a whole pot, and it is delivered into its final form in the manner least likely to cause mixture of the different parts.

7. With flint glass many circumstances are altogether different. Oxide of lead enters into its composition to the amount of one third of its weight, or more, and by its presence gives that proportion of refractive and dispersive power, which makes the glass valuable in conjunction with crown or plate: this it does in consequence of its own powerful action on light; and it makes the glass heavy also, because of its own great specific gravity. A third property belonging to it, namely its high fluxing or dissolvent powers, it also confers upon the glass. Now these three properties are unfortunately very conducive to the formation of striæ. If the least difference in composition exists between one part and another it becomes evident, because of the great difference between the qualities of the oxide of lead and the other ingredients; and a variation in proportions which in crown or plate glass would produce no sensible effect to the naked eye, would in flint glass form strong striæ. Hence it is required that the mixture be in this case far more perfect than in the other glasses; and yet it unfortunately happens that every thing tends to make it much less so. The oxide of lead is so heavy a material, and at the same time so fusible, that it melts and sinks to the bottom, leaving the lighter materials to accumulate at the top: and so imperfect are the means of mixture, under ordinary circumstances, that glass of very different specific gravity is procured from the bottom and top of the same crucible. The following are some cases of this kind, from pots containing glass not more than six inches in depth, made from the usual materials, and retained at a full heat for twenty-four hours:—

Top.....	3.38	3.30	3.28	3.21	3.15	3.73	3.85	3.81	3.31	3.30
Bottom...	4.04	3.77	3.85	3.52	3.80	4.63	4.74	4.75	3.99	3.74

These differences are great, and selected for illustration; but from appearances there is little reason to doubt that the same state of things, though not

to such an extent, occurs in every pot of flint glass made in the ordinary way.

8. Another curious illustration of the predominance of oxide of lead at the bottom is shown in many of our specimens, which have been broken through vertically: they have been affected by sulphuretted vapours and tarnished; but the tarnish has occurred only at the bottom, where the lead is abundant, and is there very strong, whilst there is no appearance of it towards the top.

9. Whilst the crucible is in the condition described, it is clear that all those circumstances, as currents, bubbles, &c., which tend to mix the glass, form abundant striæ and veins of enormous strength, and do harm unless they are continued in activity until the mixture is nearly complete; a state rarely if ever acquired in the ordinary flint glass pot. But even if this could be the case, there is a constant cause of deterioration, arising from the highly fluxing and dissolving quality given to the glass by the oxide of lead. In this respect, flint glass far surpasses crown or plate glass, and it is also during one stage of its preparation more fluid: it consequently is continually exerting a solvent power upon the crucible to a considerable extent, occasioning that very irregularity in composition which produces striæ, whilst the comparative levity of the matter dissolved at the sides and bottom, and the ascending currents at the hottest parts of the crucible are constantly mixing this deteriorating portion with the general mass.

10. The difficulties which are thus introduced into the manufacture of flint glass fit for optical uses appeared to the committee, who, however, were none of them practical glass-makers, to increase, as the scale upon which the inquiries were carried on diminished: and the enormous expense of large experiments,—the time required for each,—the number necessary to give that experience which should render any one who undertook the charge of this part of the inquiry an ordinary practical workman,—and the uselessness of the resulting glass for any other purpose than the one directly contemplated,—compelled the sub-committee to consider seriously on the possibility of making other glasses than those ordinarily in use, which, at the same time that they had the high dispersive power enabling them to replace flint glass, might have also such fusibility as would allow of their being perfectly stirred and mixed, and might be

retained, without alteration, in such vessels as could be procured of any desired size.

11. The borate of lead, and the borate of lead with silica, were the substances which, after some trials, were found to offer such reasonable hopes of success as to justify a persevering series of experiments; and the metal platina was looked to as the material out of which to form the vessels intended to be used. It was soon ascertained that the borate of lead could be readily formed from dry materials, and that silica might be added with great advantage to the resulting glass; a range of proportions between the three ingredients being permissible, which gave much command over the properties of hardness, colour, weight, refractive and dispersive power, &c., and yet remained within the required range of fusibility. Platina also was ultimately found to answer perfectly the purpose of retaining the glass: for though at first it was continually liable to failure, yet it was ultimately ascertained that neither the glass nor any of the substances entering into its composition, separate or mixed, had the slightest action upon it. Finally, it was found that several kinds of glass formed of these materials, were in their physical properties fitted to replace flint glass in the construction of telescopes, in some cases apparently even with advantage; since which time the experiments have been unremittingly pursued.

12. The great proportion of oxide of lead in these glasses rendered attention to very minute points essential; for otherwise striæ were inevitably formed, and even the destruction of the apparatus involved. For this reason, after a certain number of trials upon composition had been made, one unvarying set of proportions were adopted, and the attention given altogether to the discovery and establishment of a process which should yield constantly good results. This, as far as it has been carried into effect and proved, it is now my object to describe.

13. The glass with which I have principally worked is a silicated borate of lead, consisting of single proportionals of silica, boracic acid, and oxide of lead. The materials are first purified, then mixed, fused, and made into a rough glass, which is afterwards finished and annealed in a platina tray.

14. *Purification of materials. Oxide of Lead.*—The oxide of lead at first used was litharge; but this source occasioned frequent destruction of the platina trays, in consequence of the existence of particles of metallic lead, which

alloying with the platina, rendered it fusible. When red lead was substituted for litharge, the same effect took place, due to the presence of particles of carbonaceous and reducing matter. Both these substances also contained so much iron and other impurities, as to give a deep colour to the glass, far beyond what was expected from the quantity of impurity present; this was afterwards explained. Carbonate of lead was also found to be too impure. Finally, all the oxide of lead necessary was purified, by being converted into a nitrate, and crystallized once or twice, as occasion might require.

15. For this purpose litharge is first washed, by which many black carbonaceous and ferruginous particles are separated; it is then dissolved in diluted nitric acid, so as to form a hot saturated solution, the operation being performed in clean earthenware vessels. Both the perfectly pure and the moderately pure acid have been tried without any sensible difference in the results: a little sulphuric acid does not seem injurious; and I find that sulphate of lead will dissolve perfectly in the glass; but muriatic acid has been always avoided. As the acid, water and litharge are made to act on each other by heat, either purposely applied or resulting from the chemical action going on, it will be found that when approaching towards neutrality the liquid will become very turbid. The hot saturated solution is then to be poured from the remaining litharge and undissolved nitrate of lead, and after standing a few moments, again poured from the sediment, and set aside to crystallize in a cool place. Before it is left, however, it is to be examined as to its acidity: if strongly acid to litmus paper, it is in a right state; if not, a little nitric acid should be added, for the crystals of nitrate have always been compact and pure under such circumstances, and more readily separable from insoluble matter.

16. After eighteen or twenty-four hours, the basins of crystals are to be examined; the clear mother liquor carefully poured off, the crystals broken up in the basins; and then repeatedly washed in fresh clear portions of the mother liquor, that any insoluble deposited matter may be removed. There will generally be a portion of this deposit; but if the process has been well performed, the crystals will be quite free. If they appear perfectly white or bluish white, they need not be recrystallized; but if yellow, they must be dissolved in water, a little nitric acid added, and the crystallization repeated. The nitrate in the mother liquors and washings should be purified by repeated processes.

17. The good crystals are to be washed in three or four waters, to remove the last portion of deposit and adhering soluble impurities: but to prevent excessive solution of the nitrate, the same portions of water may be used for several basins of crystals washed at the same time, by making it pass from one to another in succession. Being thus cleansed, they are to be drained, put over the sand bath, stirred and dried, and finally preserved in glass bottles. By this process much iron and sulphate of lead are excluded; and the purified nitrate is found to yield a glass very far superior in colour to that prepared with the ordinary oxides of lead, and to exert not the slightest action on the platina: its use put an end to all the accidents and failures which resulted from the presence of metallic lead in the oxide. 166 parts by weight are to be considered as equivalent to one proportional or 112 parts of protoxide of lead.

18. *Boracic acid.*—The boracic acid for these experiments was obtained pure from the manufacturer, but before being used was carefully examined. It was rejected unless it was in white or bluish white crystals, clean and entirely soluble in water. Its solution was tested for iron by the ferro-prussiate of potash and a drop of sulphuric acid, and also for other metallic impurities by a little solution of sulphuretted hydrogen. An ounce or two were heated and dissolved in a little water; and when cold, the soluble part separated and examined for sulphuric acid, by a few drops of nitrate of baryta and a little nitric acid. It was also examined for soda by dissolving three or four ounces in hot water, adding ten or fifteen drops of sulphuric acid, and allowing the whole to cool and crystallize, expressing the mother water from the crystals; concentrating it; again crystallizing, and then acting upon the mother liquor obtained at the second time by strong alcohol, and continuing to wash with the latter fluid until all was dissolved, or an insoluble part left. If the latter circumstance occurred, the insoluble substance was examined for sulphate of soda, which if in any sensible quantity occasioned the condemnation of the boracic acid. The care respecting alkali in boracic acid was taken in consequence of observing certain bad effects produced in glasses which appeared referrible to its presence.

19. When the boracic acid was acknowledged as pure, 36 parts by weight of the crystals were considered as equivalent to 24 parts, or one proportional of the dry substance.

20. *Silica.*—This material is in its most convenient state when it forms part of a combination consisting of two proportions silica, and one oxide of lead. As yet, the silica I have used has been the flint glass-maker's sand, obtained from the coast of Norfolk, well washed and calcined. The silicate has been prepared by mixing two by weight of this sand with one of litharge, or with such quantity of nitrate of lead as is equivalent to one of litharge (16); the mixture is put into a large Hessian or Cornish crucible, which being covered over, has been put into a furnace and raised to a bright red heat for eighteen or twenty-four hours. On taking out the crucible, the charge has been found diminished somewhat in bulk, and of a porous structure and appearance like loaf-sugar. It has been freed from the crucible, the outside portions removed, and the pure parts carefully pulverized in a clean Wedgwood mortar. The powder has then been washed over in water, so as to obtain the whole in a fine state of division; after which it has been dried, and preserved in bottles. No sieve should be used in these comminuting operations, nor any reducing or metallic matter brought in contact with the substance. Every care should be taken to avoid contamination: 24 parts by weight of the silicate are equivalent to 16 parts, or one proportional of silica, and 8 parts of protoxide of lead.

21. The advantage of the silica in this combined state depends upon the known composition of the substance, its comparatively easy pulverization, and ready fusion with the other materials. That there is iron in the silica (and the litharge when used) is objectionable; and the trials for its removal have only been delayed that the investigation of a more important point, namely, a successful process, might proceed. From some brief experiments, I am led to believe that an unexceptionable source of silica will be obtained by acting upon this silicate, in a state of fine division, by nitric acid and water, or else by the use of rock crystal.

22. On some occasions I used pulverized flint glass, as the source of silica, conceiving that being already in a fusible state, it must possess an advantage over other silica, in allowing rapid mixture with the other materials. Allowance was made for the oxide of lead present, and the alkali was permitted to pass, as a substance that would probably do no harm. But a striking effect took place, which at once showed the necessity of perfectly pure materials. The glass when finished and cold was of a deep purple colour: this was immediately

referred to the manganese in the flint glass; a supposition proved by repeating the experiment with other flint glass, and then with flint glass of our own manufacture in which no manganese was used: the latter glass gave no purple colour; the former, a colour as deep as the first flint glass.

23. Thus it appears that this very heavy glass, the silicated borate of lead (and I find it to be the case with other heavy glasses), has the power of developing the colour of mineral substances far beyond what flint glass possesses; just as flint glass surpasses in the same property plate and crown glass. In the case in question, the manganese, which did not give a sensible tint to the flint glass, produced a strong colour when diluted eight or nine times by the heavy glass, for the proportion of flint glass used was only $\frac{1}{11}$ths of the whole. On making a few experiments with iron, I find that the same strong development of colour is produced with it in these heavy glasses; so that the utmost care is necessary to preserve all the materials during their preparation, and the glass in every part of the process, from metallic contamination.

24. The use of flint glass even without manganese was also objectionable, because of the alkali in it, which, as before stated, was found to produce bad effects, and rendered the glass containing it very liable to tarnish.

25. Such are the materials from which the heavy optical glass has been latterly manufactured. When the composition has been determined upon, the proper proportions and quantities of each are weighed out in a clean balance and vessels; thus, for the silicated borate of lead glass, consisting of single proportionals of each substance, 24 parts of the silicate would be taken, for they contain a proportional of silica equal to 16 parts, and in addition 8 parts of protoxide of lead: the proportional of oxide of lead has been taken as 112 parts; but there being 8 in the silicate, the quantity of nitrate of lead equivalent to 104 parts only are required, and this is 154.14 parts: the equivalent of dry boracic acid is 24, which being contained in 42 parts of the crystals, that quantity is the one required. These proportions when heated and submitted to mutual action will leave only 152 parts of glass, or thereabout, for

154.14 nitrate of lead contain	104	protoxide of lead.
24.00 silicate of lead contain	8	ditto.
	16	silica.
42.00 crystallized boracic acid contain	24	dry boracic acid.
	152	glass.

Hence the materials for any quantity of glass can be easily calculated; and if the above parts be ounces, about 9lbs of glass will result. The nitrate of lead is then to be broken small in a clean mortar, and the other ingredients well mixed with it in basins, the use of metal or dirty implements being carefully avoided.

26. The mixture is next melted, and made into rough glass. This preparatory operation is necessary, because from the quantity of vapourable matter which is disengaged in this part of the process, the materials, if put at once into the finishing vessel and furnace, might boil over and do injury; and the acid nature of the vapours themselves, if it did not occasion harm by acting on neighbouring iron and other parts of the furnace, would at least cause inconvenience. It is effected in a furnace, which will be particularly described in the Appendix to this paper. It will be sufficient here to state, that being a close furnace, the part immediately beyond the fire-place forms a horizontal chamber, covered above by an iron plate having large circular holes; these allow crucibles to pass through them, and to stand supported on the bottom of the chamber, whilst their edges rise above the upper iron plate. In this way the fire is applied very generally to the crucibles, whilst their mouths are altogether exterior to the furnace, so that the introduction of any reducing or colouring impurity from the fire is prevented, and the greatest facility in introducing the mixture, of watching its fusion, of stirring the glass, and finally of ladling it out, is obtained. The holes through which these crucibles are inserted are five or six in number; they are never all in use at once, and those out of use are covered by crucible covers. The heat is not given altogether by flame; but whilst coal is used in the fire-place, coke is applied between the crucibles, being introduced for that purpose, and arranged through the unoccupied holes. The iron top of the furnace is covered by a second iron plate, or, what is better, by earthenware plates, to retain the heat. The crucibles are of pure porcelain ware, and as thin as they can be obtained. The covers for them are evaporating dishes, considerably larger than the mouths of the crucibles: being turned upside down, they rest, when in their places, upon the neighbouring earthenware plate; not touching the crucibles, but preventing any thing from falling into them, and preventing the vapours from passing into the room. The latter are by the draught of the chimney drawn through by the sides of the crucible into

the furnace, and carried away up the flue, so as to occasion no annoyance to the operator. The covers are slung by a piece of platina wire, which being passed across the middle on the outside, is bent at each end round the edges, so that a rod of iron slightly curved at the extremity, easily suffices to remove them when the crucible is to be opened. Great care is always taken to put them in clean situations, and that in their removal nothing shall fall from them into the glass.

27. This furnace is found to be very effectual in its action; being connected with a high flue governed by a damper, great command of the temperature is obtained. The crucibles before being used are examined as to soundness, and then their temperature is raised gradually, and should not be above a dull red heat when the operation commences. The mixture already described (25) is then introduced, and the crucible covered; decomposition of the nitrate of lead instantly commences; the boracic acid loses its water, all the fixed elements unite; and it is remarkable that though a considerable quantity of boracic acid usually sublimes with the water when the latter is driven off from its crystals unmixed with other substances, yet scarcely a trace seems to evaporate in the present instance, in consequence of the presence of the oxide of lead.

28. The heat should not be raised too high or the operation hastened, and then the ebullition will proceed very gradually and favourably, the rough materials being by degrees converted into glass. Before the first charge is entirely melted a second is put in, and when that is fused down, sometimes a third, according to the quantity of glass present and the soundness of the crucible. When all is fused, the temperature is allowed to rise, but not too much, lest action upon the crucible to a serious extent should occur; the glass is then well agitated and mixed by a platina rake or stirrer, to be described hereafter. Finally the glass is either transferred by a platina ladle into trays roughly turned up out of old platina foil, or into a clean deep white earthenware vessel containing much distilled water. In the lattter case it is obtained in a divided state, and when drained, is dried on the sand-bath, and put up in clean bottles.

29. When a crucible has been emptied of its first portion of glass, it will serve, if carefully used, for a second, third, fourth, or for many operations; but

it should be watched for cracks and casualties, that the running of the glass into the furnace may be prevented, and, if necessary, another vessel taken.

30. The rough glass thus prepared is in the next operation to be converted into an annealed and finished plate. The size must therefore be determined upon, and we will assume it as 7 inches square, and 8 tenths of an inch thick, that being the dimension of the largest plate as yet made. For the purpose of making a competent platina vessel, a plate of that metal will be required at least 10 inches square; but if larger, it should not be cut, but either made into a tray with higher sides than is absolutely needful, or else used first in the manufacture of a larger plate of glass than the one to be described. It should be of such thickness as to weigh at least 17·5 grains to the square inch; and it is important that in its preparation a good ingot or the good part of an ingot of platina has been selected, and that it has been rolled very gradually and carefully without the formation of any holes by the adhesion of dirt or hard particles, or by the dragging of the metal in the mills. The desired perfection is, I understand, best obtained by rolling the platina between two clean plates of good copper.

31. The plate, being laid upon clean paper or a cloth on a smooth table, is to be cleansed with a cloth and a little water or alcohol, and then to be ignited at every part by a large spirit lamp. It must next be carefully examined as to its state and the occurrence of places upon its surface where holes are likely to exist. If the metal seems dragged in any place, an effect indicated by a roughness upon the surface, or by short lines parallel to each other but perpendicular to the course of rolling, such place should be noted or marked, for which purpose a dot of ink will be convenient. If a scale appears, or a small portion is apparently folded over, it should also be marked; and if a black spot is visible, (and they are sometimes formed by the adhesion of a particle of dirt or grit,) it should be examined, and removed by the point of a knife, if necessary, and its place also marked. All these places and the whole surface of the plate should then be examined for holes by a still stronger test, namely, by holding the sheet of metal before and close to a bright light, as a candle or lamp, in a dark room, and every hole observed marked. In making this examination, it must be done carefully and minutely, holding the plate in different directions to the light (for sometimes the holes are oblique),

and being careful that no reflection from illumined objects, as the hands, on to that side towards the face shall give deceptive indications. In the marking, too, the indicating spot should always be made at a certain distance from the hole, as the fourth or the third of an inch, and on the same plate constantly in the same direction or towards the same edge; the holes are then easily found again, and the mark remains during the soldering to guide the operator.

32. The holes discovered by these examinations are to be closed by little patches of platina soldered with gold; for gold, like platina, may be safely used in these experiments, when reducing matter is absent. The gold has been used in the finely divided state in which it is obtained by precipitation from its solutions by means of sulphate of iron, but it must be washed perfectly pure: the patches are formed by cutting a piece of clean new platina foil into small square or rectangular plates: a sufficient heat can usually be obtained by the use of the spirit lamp and mouth blowpipe. In the process of soldering, a little of the powdered gold is heaped upon the hole and slightly flattened by some clean instrument, the spirit lamp is applied underneath for a moment, which causes the gold to adhere slightly, a selected patch of platina is laid delicately upon the gold, and then the heat of the spirit lamp, urged by the blowpipe, is directed beneath against the place. Usually the gold will melt and run instantly, the platina patch will come into close contact with the plate, and the operation will be completed. If well done, the fused gold will appear all the way round in the minute angle formed by the edge of the patch, and also faintly at the hole on the opposite side of the plate.

33. Sometimes when the patch is large, or in the middle of a plate, the heat obtained as above is hardly sufficient to melt the gold freely and cause perfect adhesion. In such cases, a single or double piece of platina foil loosely laid over the part, prevents loss of heat from the upper surface, and frequently causes such increased elevation of temperature as to render the soldering perfect and effectual. In the few cases where this expedient has not succeeded, I have resorted to the oxyalcohol blowpipe, using a small bladder of oxygen with a little attached jet for the purpose. This has never failed to produce an effectual heat, and 15 or 20 cubical inches of oxygen are sufficient for many operations.

34. This application of patches and soldering is only secure for small holes,

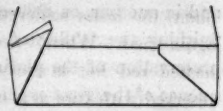
i. e. such as a pin might pass through, and smaller. The patches are always to be applied on that surface of the plate which is to constitute the outside of the tray; and therefore, before the soldering begins, the two surfaces should be examined, and the most polished and perfect selected as that intended for the inside. The patches are valuable in their use far beyond what the mere application of gold to the hole would be; for the heat afterwards applied to the tray when charged with glass, is abundantly sufficient to melt gold; in which case, if unsupported by the platina patch, the weight of glass and the action of stirring would probably force the gold out of the hole and cause the tray to run; whereas the patch of platina, although the gold holding it to the plate is liquid, still adheres by so strong a capillary action as to be sufficient to retain its place, and being outside is not disturbed by the motion of the stirrer. Besides, after a long application of heat, the gold and platina combine so perfectly as to become one piece of white alloy, infusible at the heat applied.

35. The plate is now to be folded into a tray, preparatory to which, a piece of thin board is to be provided as a guage, which in the present instance must be 7 inches square. This laid upon the plate and held tightly down, directs the foldings of the sides, and would, if placed in the middle, leave sufficient for edges one inch and a half high all round; but as the plate should serve for use several times, it is advantageous to apply the guage a little eccentric; for then, when used for a second and third operation, its place may be shifted, and the folds not occurring where they did before, there is less chance of holes being broken through the platina. The folds necessary at the corners of the tray are especially likely to render the same parts unable to bear a second and third bending; but the necessity of having them in the same place may be usefully obviated by placing the guage oblique to the sides in one direction and in another, on different occasions, and moreover gives other advantages in finishing the folding of the corners (36). These attentions, tending to the preservation of the platina for repeated service, are very needful, in consequence of the great expense of the material: the value of the plate in question is about 6*l.* 10*s.*, and when worn out, it may be sold for about half that sum. Whether it be used therefore once, twice, thrice, or four times, makes considerable difference in the expense of the resulting plates of glass.

36. When the guage is properly placed on the platina, the sides are raised

perpendicularly: this produces four projecting folded triangular corners, which being pressed close, are then turned against the sides, and a square tray is finished, which has no aperture or orifice below its upper edge. The folding of these corners is a matter of much more consequence than might be anticipated. The plate is seldom so regular that the parts of two neighbouring sides which come together at a corner are exactly of equal height; neither is it desirable that it should be so, and the unsymmetrical position of the guage to the plate, already recommended (35), is almost sure to prevent it. In that case, of the two sides of the folded corner, one will be higher than the other, and if the corner be so folded that its lower side is towards the tray and beneath its edge, a kind of syphon is formed which becomes charged with fluid by capillary action, and continues to discharge glass from the tray during the whole time of heating, notwithstanding that all the edges are much above the level of the fluid within. This in a long experiment is competent to occasion serious injury.

37. I have found, even when the edges of a corner have been of equal height, but below the edge of the side against which they are disposed, that still this capillary and syphon action has gone on, and the reason is not difficult to comprehend; the corners therefore have always been folded in such a manner, that their highest edge has been inwards, and both their edges above the level of the corresponding edge of the tray. To effect this, the line of their lateral flexure is not perpendicular to the bottom of the tray, but a little outwards above, and the proper degree of inclination is easily given by using a mould upon which to bend the corners. This should be a thick square piece of wood, having the four corners cut with different degrees of obliquity: when the corners of the tray are first imperfectly formed, it will be easy to ascertain by trial, which corner of this mould will give the obliquity and position already described as necessary, after which the folding may be easily finished upon it. The accompanying sketch represents first a good and then a bad folding.



38. All occasion for changes in the folds, especially at the corners, should be avoided. The folds should be decided upon as the work proceeds, so advantageously as to make alterations unnecessary. The closer the corners

are pressed, the smaller is the quantity of glass contained in them, and the less risk is there of the platina being broken when the finished glass is taken out; but it is proper to avoid general contact between the corners and the sides against which they are disposed, otherwise welding is likely to occur during the stirring, and the platina is injured for future experiments.

39. The tray being formed is again to be examined for holes, first by a light as before (31), and then in the following manner. Being laid upon a sheet of bibulous paper, alcohol is to be carefully poured in until the fluid is within the fourth or the sixth of an inch of the lowest edge of the tray, so as to occasion no running over at the sides or corners. If a large hole exist, it will be rendered visible immediately; but if none such appear, a large basin or some other cover is to be placed over the tray to prevent evaporation, but without touching the vessel or its contents; and the whole is to remain undisturbed for some hours. Being then examined, the wetting of the paper will indicate a hole or a badly folded corner, and will point out the faulty place: the tray may easily be shifted from one part of the paper to another for the discovery of any moistened places beneath. Sometimes holes occur so small that alcohol will not run in a sensible quantity through them. Suspected places of this kind and suspicious corners also should be examined by a clean dry point of bibulous paper, which soon shows, by its change of appearance, the transmission of any fluid: but attention is required that no false indication be produced by carelessly bringing the paper near the upper edges of the platina, especially in the folded places. These minute holes do not occasion much harm in the furnace, but no fault should be allowed to pass which care can correct.

40. When the tray is faulty, the alcohol must be removed by a small syphon, the holes soldered in the manner before described (32), and the tray again tried. When it proves good, it is, after the removal of the alcohol, to be heated red hot in every part by the flame of a large spirit lamp, and then reserved with care in a clean place until required.

41. If the platina has been used before, it should first be ascertained that none of the glass from the former experiment remain on it. If there be any portion, the plate must be returned to the weak acid or pickle out of which it has been taken. If free from glass, it should then be examined as to any

chemical injury it may have suffered. Any part which is altered in appearance, or has been attacked by the acid, or which tarnishes when heated to redness by the spirit lamp, has been thus affected; and it will depend upon the extent of the action whether the plate is unfit for further use. No chemical injury is occasioned by the proper and successful performance of an experiment.

42. An examination for holes by the candle or lamp must next be made, especially in the folds at the corners and where adhesion of the platina from welding may have occurred, and any that are discovered are to be marked as before (31). The plate should then be flattened by being put between two sheets of writing paper upon a smooth table, and the edge of a folding knife or some other smooth substance drawn over it; but if this be done whilst old glass adheres to the plate, it is almost certain to produce injury. The holes are then to be soldered and mended, the patches being applied upon the same side as before. The guage for the new tray is to be applied to the plate, shifted, if there be occasion, from its old position, as before intimated (35), and the folding of the tray, its completion and examination, to take place as before.

43. It is desirable never to cut the platina smaller than can be helped, but always to make the largest plate upon it for which it is competent. Then, when operated with a second or third time, smaller guages may be used, and the folds will not be repeated in the same place; and if injury occurs to the metal, being generally at the sides of the tray, the middle part will still be left for the preparation of smaller plates of glass.

If such large plates of platina are required for trays as can hardly be rolled at once, there is no difficulty in making a folded joint and rendering it tight by soldering with gold.

44. A kind of furnace, unlike the former, is now required for the completion of the glass, and its delivery in the state of an annealed plate. This furnace shall be described accurately in the Appendix. It may here be sufficient to state that it consists of a fire-place in which coals are burnt; of a part beyond, acting both as furnace and flue, in which coke is used; and of a chamber above, to be heated by the fire, though out of the course of both flame and smoke. It is in this chamber that the glass is made; so that, by the arrangement adopted,

at the same time the substances are fused and access for stirring allowed, the essential condition of excluding impurity or reducing matter is also fulfilled.

45. The fire-place itself is of the ordinary construction, and fed with fuel by an aperture in front in the usual way. I have found abundant reason to be satisfied that the passage of steam beneath the bars of the grate is of considerable use; for which reason an iron trough charged with water occupies the lower part of the ash-pit. The bars are by this arrangement preserved very cool and do not burn away; they are easily kept open and clear of clinkers; the free passage of air to the fire is permitted; and the action of the furnace retained at a high point for any number of hours together.

46. That part of the furnace beneath the chamber requires peculiar and careful arrangement; for at the same time that such a heat as will soften the neighbouring materials is produced there, the bottom of the chamber in its softened state and charged with several pounds of materials, has to be firmly supported for many hours together without change of position.

47. The coke necessary in this part is introduced by two or more holes in the side of the furnace, which, when necessary, are stopped by bricks. The bottom of the chamber is supported on ledges at the sides, and upon the ends of fire bricks in the middle, firmly placed at intervals so as neither to stop the passage for smoke and flame, nor the cross passages for the introduction of coke.

48. The value of the coke arrangement in this as in the other furnace is very great. The heat obtained by the united action of the coke and the flame from the fire-place, is abundantly sufficient; and whilst obtained at the necessary point does not involve that degree of mechanical action required for stoking and stirring, which is necessary with coals, and would risk the destruction of the soft thin bottom of the glass chamber. It further occasions the perfect combustion of the smoke produced in the coal fire, which at first was so considerable in quantity that, had it continued unaltered, the experiments must have been removed from the Royal Institution; in which case they would probably have been discontinued altogether.

The flue is the same as that connected with the former furnace, and has a damper for regulating the heat, especially useful during the annealing operation.

49. The chamber was at first of cast iron, that material being selected as one which would bear a sufficient temperature without melting, would conduct and transmit the heat freely to the substances within, and could be easily obtained of the requisite form. The upper aperture was closed by plate iron covers, and in the first trials all appeared to answer well; but when large experiments were made, and the heat was continued for a long time, the bottom gave way and became irregular; and upon endeavouring to rectify this, and place the tray of glass level by means of sand, the transmission of heat to the glass was prevented, the temperature of the iron rose, and the bottom melted. Besides these injurious liabilities, if the smallest portion of glass passed out of the tray, the moment it touched the iron it was reduced, the lead immediately caused fusion of the platina, and in an instant the tray was destroyed, the experiment stopped, the glass rendered black and useless, and the bottom of the chamber covered with lead and rendered unfit for another operation.

50. Finally, one very curious action of the iron was discovered, which immediately caused its rejection. Plates of glass, which seemed very good in other respects, were frequently so discoloured by dark smoky clouds as to be useless. These could not be referred to any impurity which had been left in the materials or had entered accidentally, and, as the platina was in all such cases altered and injured, was at first supposed to be occasioned by some particular action exerted between it and glass at high temperatures. But upon every fair trial to verify such chemical action, the proofs failed, however high the temperature used, or however minutely the metal was divided. At last the cause was discovered. To understand it, it must be known that the platina tray, with the glass in it, was either placed directly upon the bottom of the iron pan, or, for greater security, with only a plate of platina intervening; and that the whole was covered by an evaporating basin turned upside down, forming a sort of inner chamber within the large one. In this confined state the oxygen of the portion of air present was soon abstracted by the heated metal, an oxide of iron being formed in consequence, and at the same time also, a portion of carbonic oxide from the carbon in the cast iron. At the high temperature to which the experiment was raised, this carbonic oxide was competent to reduce a portion of the oxide of lead in the glass to the metallic state, itself becoming

carbonic acid; but as soon as the carbonic acid so produced came in contact with the heated iron, it was again converted, according to the well known condition of the chemical affinities at these temperatures, into carbonic oxide, and went back to the glass to repeat its evil operation and produce more metallic lead. In this way it was that the glass became sullied by smoky clouds consisting of metallic lead. It was the lead thus evolved also, that, by alloying with the platina, had produced the appearance of chemical action always visible in these cases; and now I knew how to account for the failure of many experiments in consequence of the formation of holes in the trays in a manner before quite inexplicable: for in the experiments purposely made to investigate this point, sometimes the glass was darkened only at the surface, the lower part being quite clear and good; and then, though the platina tray was frequently cut through as with a knife all round level with the surface of the glass, it was quite unaltered below. At other times the superficial stain was in a greater quantity, and had collected together into little drops like fat upon hot water, and upon examination each little globule was found to be soft metallic brilliant lead. At other times a much larger globule hung from the middle of the surface into the glass, barely sustained there, and ready to sink by the least agitation when in a heated state, and in some instances the bottom of the tray was alloyed and perforated by globules of lead which had thus been formed and deposited, and the glass just running out, whilst another globule was in progress of formation at the surface exactly over the place of the hole.

51. When iron was dismissed as the material of the chamber, earthenware was resorted to. The sides were built up of brick, and the bottom formed of tiles, which resting at the sides upon ledges, and at the middle upon the fire brick supports (47), could be replaced at pleasure. The same iron covers were used for the upper aperture of the chamber as before.

52. The use of earthenware as the material, made it far more difficult to apply a sufficient heat to the contents of the chamber than before, because of its inferiority to the iron as a conductor of heat; and a series of investigations were required to discover that substance, which, at the same time that it had sufficient strength and exerted no injurious influence, was also a sufficiently good conductor. Reigate fire-stone, recommended by the builders, did not answer the purpose, and moreover in thin plates was liable to fuse and slag.

Slate, however carefully heated, shivered and split, not only across, but parallel to its structure, and then, as soon as air intervened, it transmitted too little heat. It also softened, became curved, and let in air and smoke, and at last gradually fused, becoming unable to bear the weight of a large experiment. Yorkshire stone rubbed down into plates $\frac{1}{8}$ths of an inch thick, answered moderately well, if the application of heat was carefully made and gradually raised. It cracked in a few places, but did not fall to pieces; and it was more difficult of fusion than the former substances. Fire tiles of various kinds were tried; those made of Stourbridge clay answered the best, and when about $\frac{1}{8}$ths of an inch thick and carefully heated, might be successfully used; but that which we finally arrived at was the use of plates made of the materials from which Cornish crucibles are manufactured. These we obtained through the intervention of our President; they were purposely manufactured for us by Mr. Michell of Caleneck in Cornwall, a gentleman who has been ever willing and anxious to assist us in our inquiries, by supplying us with vessels of any size or form, or any other article which it was in his power to produce.

53. The Cornish plates have not much cohesion, and feel tender in the hand. They may be rubbed down to a flat surface, and resist any heat which can be applied to them in these or in much more powerful furnaces. They are therefore readily brought to any thickness, and when of about $\frac{1}{8}$ths of an inch, and supported in the furnace as before described (47), have strength to bear any weight required to be placed upon them. They do not crack, nor do they force themselves to pieces by expansion; but they are porous, as indeed are in a greater or smaller degree all the materials of which the chamber and its sides are now composed.

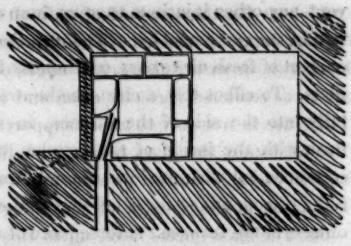
54. The porosity of these materials was of great importance; for it allowed of the passage of gaseous matter, and that even of a reducing nature, from the fire into the chamber. I have frequently had evidence that the sides and bottom might be considered as a very sieve-like partition between the fire, the flue, and the space called the chamber; for when the upper aperture has been closed, there has been a current through the chamber in the direction of the flame, the gaseous matter entering at the extremity nearest the fire, and passing out at the end towards the flue. In one or two cases, oxide of lead was actually reduced, and the glass thus rendered cloudy.

55. Hence it became necessary to use some certain means of maintaining an oxygenating atmosphere about the glass; to obtain which, and also to prevent any other injurious vapours from the fire entering the space beneath and within the earthenware covers (50), the expedient was adopted of allowing a current of fresh air to pass continually into that space and circulate about the glass. To effect this, a clean earthenware tube, glazed within, was let horizontally into the side of the furnace, in such a manner that one extremity was flush with the inside of the chamber, and of such height, that its lower edge corresponded with the level of the bottom upon which the glass in its tray was to be placed, whilst the other end of the tube reached to and was flush with the outside of the furnace. A loose piece of tube, similar in kind but smaller in diameter, being laid upon the bottom of the chamber, and applied at its end to the orifice of the larger one, served as a continuation of it until the inner extremity reached to and was under the cover of the glass experiment. When the furnace was hot, there was always a draught inwards through this tube; but the quantity of air admitted was regulated by a valve (70). The air, by first passing through the hot sides of the furnace, then through the shorter ignited tube serving for connection, was transmitted in a thoroughly heated state to the place where its presence was required, without producing any serious cooling effect; it there maintained a continually oxygenating atmosphere, and, judging from the effects, prevented the draught inwards of any vapours from the fire to the space beneath the glass covers.

56. The next point of importance, in the preparation of the glass, is the arrangement of the tray in the furnace, whose powers have just been described. To understand this, it will be necessary to say that the glass chamber is 25 inches long, 13 inches wide, and 8 inches deep, and that the fire being at one end, the flue is at the other. Plates of glass 7 inches square have been made in it; but it would probably require a larger furnace to make much larger pieces.

57. The bottom of the chamber being perfectly level and clean, the guage board, on which the tray was formed (35), should be placed on the middle of the half next the fire, and then a piece of connecting air tube taken, which being laid on the bottom of the chamber, may extend from the fixed air tube by the side of the guage as far as the middle, or even towards the other side of the chamber. After this, pieces of Cornish tile (53), or other clean

earthenware which will not fly in the fire, contains but little iron, and is free from glaze, are to be prepared, of such size that they will fit in loosely round the guage, covering the rest of that half of the chamber bottom, and serving to support the sides of the tray when in its place. This support to the tray is highly needful; for, otherwise, the weight of the glass, and the action of stirring, would be more than the thin and heated platina could support. The thickness of the pieces should be, for the plate in question, about 1 inch, and they should be all uniform in that respect. They should never rise so high as the edge of the platina, lest glass should accidentally pass from the tray to them, or impurities from them to the glass. An excellent guide to their thickness is, to make it similar to that of the intended plate. When they have been roughly arranged around the guage, the latter should be withdrawn, and the tray itself introduced, the pieces being now finally adjusted about it. They should not be so arranged as to press against its sides; but the latter should be at liberty, though only so much, that upon the least tendency of the sides outwards, they should be supported by the pieces. The assistance thus given should be directed rather to the sides than the corners, and it is better that the latter should not be in contact with these adjuncts, but be allowed to sustain themselves, for they are strong enough for the purpose, and the corners are always those places at which, from one circumstance or another, the glass is most likely to pass outwards.



58. The piece of earthenware which is fitted nearest the mouth of the air tube should have its angle taken off, or some other provision made, as by making the orifice of the tube oblique, that the passage of air may be uninterrupted; and on that side the tube itself may frequently form the support to the tray. If it does, and is glazed on the exterior, a piece of loose platina foil should be wrapped round it at the part where it touches the tray, to prevent adhesion by the glaze when cold. The general disposition of the tray, the tube, and the packings, may be seen in the sketch above.

59. When the first set of packing pieces is properly adjusted, a second series is to be arranged over them; but these are to be removed backward from the tray about the third or half of an inch all round, that accidental contact with its edges may be avoided. Their thickness should be sufficient to raise them level with, or rather above, the edges of the tray. All these adjusting pieces are to be rendered perfectly clean and free from dust before they are applied. Their use is not only to afford support and assistance to the platina tray, but also to sustain the glass covers, and likewise, by retaining the heat upon the bottom of the chamber, prevent much of the inconvenience that would otherwise occur at the times of stirring the glass.

60. The glass covers have, up to this period, consisted of inverted evaporating basins, suspended at pleasure, in the manner before described, by platina wires (26). When the platina trays used have been sufficiently small to admit of the arrangement in our present furnace, two, and even three covers have been used simultaneously, each prepared with its own platina suspension; but of such size, that the larger could be placed over and inclose the smaller, without touching it. In such cases the temperature of the glass, after being lowered by stirring, or in any other way, rose very rapidly; but with the large plate of 7 inches square, the furnace would admit of but one glass cover of sufficient size, and the only additional assistance which could be obtained was that which was given by putting a similar but smaller cover on the outside and above the principal one.

61. The first and important cover is to be selected of such dimensions, that when in its place and resting by its edges upon the packing pieces, it shall fully inclose the platina tray and its charge, not only for the purpose of accumulating heat and confining an oxygenating atmosphere within, but also sheltering the glass, and preventing any oxide of iron from the chamber covers, or dirt from other sources, falling into it. These covers, when hot, are raised and removed by means of clean iron rods, which being sufficiently thick to have abundant strength, and no injurious degree of elasticity, are made taper at one extremity, and slightly curved there. This end is easily introduced beneath the platina suspension wire, and as easily withdrawn when the cover is removed.

62. All these matters being preliminarily arranged, the final disposition of

the tray and its charge is made. The air tube is carefully wiped, and its external aperture closed by a clean loose plug of dry sponge. The tray is for the last time freed from dust by inversion and blowing upon it, and is put into its place. The quantity of rough glass necessary for the required plate, about 8lbs in the present instance (29), is carefully weighed out, and then introduced by an evaporating basin, or some other means which shall not allow of the admission of any reducing or colouring matter, or permit any portion of glass to pass beyond the edges of the tray. The glass covers are then to be arranged in their places; the iron covers of the chamber likewise adjusted, and over all are to be placed a set of thick earthenware tiles, which have been fitted together so as to constitute a general covering to the whole, well calculated to retain heat.

63. The ensuing part of the process is one in which the precise order of, and most advantageous proceedings have not yet been ascertained. Variations have been made up to the very last experiment, and it is only by still more extensive experience that the arrangement will ultimately be settled.

64. A fire being lighted in the furnace, and some coke put beneath the glass chamber, the temperature is gradually raised. In about an hour the bottom of the chamber begins to appear ignited, and in four hours the top iron covers are usually dull red hot. These appearances are useful, as indications of the progress of the operation. When the furnace has been heated for the first half hour, then every care is taken that the temperature may be fully sustained to the end of the experiment; and besides the ordinary kind of attention to the fire, particular care is taken that coke be supplied, by the lateral holes, to the part beneath the chamber; for, if the fuel there be allowed to burn out, the heat soon falls, notwithstanding the flame from the coals. Although the fire may seem quickly to have attained its best condition, yet the temperature continues to rise in the chamber long afterwards; for, from the quantity of lateral brick-work to be heated, it is usually many hours before the sides of the chamber are so hot, that the tray and its contents can attain their highest temperature. At the same time it must be understood that the heat of the glass is very much governed, especially at the early part of an experiment, by the number of covers over it, and rises far more rapidly, and much higher, with two or three covers than with one.

65. Perhaps the glass may with propriety be examined once, early in the

experiment, for the purpose of ascertaining that the tray and its contents are safe; but usually it is left for six or eight, or a greater number of hours, that the whole may fuse, the temperature rise, and the bubbles escape. When the glass is to be examined, the tile and iron covers are to be removed from over that half of the chamber containing it, by which, consequently, the glass covers are exposed, these are next to be carefully raised, one by one, using the iron instrument before described, for the purpose (61), and, as they are removed, are to be carefully put into the further part of the chamber, which still remains covered, where they will be retained in a heated state. This prevents their cracking and falling to pieces, as they would do if brought into the open air. If the experiment, and consequently the covers, are upon so large a scale that the latter cannot all be placed in this situation, then the exterior ones may be placed upon the top of the heated covers and tiles; but the particular cover, which immediately incloses the glass, being of great importance, must be put into the further safe part of the furnace, that it may be carefully preserved from injury, and ready to be replaced over the glass with the least possible disturbance.

66. The moment the last cover is removed, the glass is exposed to any falling substance from the iron plates, or tiles, or other sources, so that extreme attention is required at such times, to keep the place free from dust, and to perform every requisite operation as quietly as possible. The current of hot air which rises from the chamber, ascending and striking against the ceiling, frequently causes, by change of temperature and mechanical agitation, the separation of small particles of matter, which, descending, cause risk of injury to the glass; for which reason, it may sometimes be needful to have a temporary shelter fixed over the furnace, either of tin plate, clean boards, or some other material which shall not throw off scales or impurities of any kind.

67. If, by any unfortunate accident, a fragment of matter does fall into the glass, it should be instantly removed. It certainly will not sink, because of the great density of the glass, and may be taken out, usually with facility, by touching it, and the glass in its neighbourhood, with the platina stirrer (28), or the bottom of the platina ladle (28). In carrying it and the adhering glass away, great attention should be given, that none of the latter fall over the sides of the tray; since such portion might be a means of introducing im-

purity hereafter, or of cementing the tray and the earthenware together in a very inconvenient and injurious manner.

68. If, also, it should be observed, at this time, that there is a superabundance of glass in the tray, and not sufficient distance between its surface and the edges of the platina, the excess should be ladled out (28), an operation easily performed, but which must be done with care.

69. When the glass is ascertained to be in a proper condition, and that there is no appearance of any portion of it outside the tray, the covers are to be replaced, the chamber closed, and the heat continued. If the covers be glazed, some precaution is required in their arrangement; for on putting the second cover over the first, if they are left in contact by a portion of glazed surface, they will be found, upon their next removal, to adhere at that place. They should never be put in contact therefore with each other, or, if that cannot be avoided, a piece of old platina foil should be laid upon the place where the contact is necessary (58).

70. Whilst the glass is covered and subjected to a high temperature, there is, as before stated, an inward current of fresh air passing continually to and about it through the air tube, during the whole time of the experiment (55).

It was necessary to apply a valve to the external orifice of this tube to regulate the supply; for the draught was so considerable, that the glass was cooled by it, and much dust carried in. Finding reason to believe that even when very much diminished, the quantity of soots and dust in a London atmosphere, and especially in that portion of it taken from an experimental room in which a powerful furnace was at work, were competent to do much harm in eighteen or twenty-four hours, by giving colour and forming striæ, experiments were made on the means of cleansing the entering air. It was found easy to effect this, by the assistance of two or three Woulfe's bottles, or two or three jars, inverted one within another, using at the same time portions of diluted sulphuric acid, or such solutions of salts in the vessels as would not supply any moisture to the air, but rather take water with the dust from it. In these cases the air did not bubble through the liquid, but only passed close to its surface, and had time to deposit its dust during its passage through the inclosed spaces above the fluid: but, finally, a still simpler arrangement was used, consisting merely of a plug of clean dry sponge fitted into the end of the tube,

which, at the same time that it allowed sufficient air to pass, seemed, from the appearance of the tube afterwards, to have excluded every impurity.

71. There are two conditions of the finished glass, each of great importance, which it is the object of the process to obtain in this state of the substance. One, and the most essential, is the absence of all striæ and irregularities of composition; the other, the absence of even the most minute bubbles. The first is obtained by agitation and perfect mixture of the whole; the latter, principally by a state of repose: so that the means required to be successful on both points are directly opposed to each other. Were the glass absolutely incapable of change by the long-continued action of heat, it would be easy first to render it uniform by stirring, and then to leave it in a quiescent state, until the bubbles had disappeared; but I am not yet fully assured of the fact which is necessary to this order of proceedings. That the glass as far as proportions are concerned, if changed at all, is altered only in an extremely minute and inappreciable degree, is shown by some experiments, in which, after a portion had been prepared and heated for many hours, and also stirred well, the resulting piece was divided into smaller portions, and these heated at different temperatures, in platina trays, for sixteen hours. Three portions were heated as powerfully as the furnace would admit of; three only to redness, which may be considered as a very low heat; and three to an intermediate degree: all were cooled slowly and annealed for an equal time. The specific gravities of each after the experiments were as follows:

Highest heat . .	5.4206	5.4211	5.4203	Mean sp. gr. 5.42066
Intermediate heat .	5.4253	5.4242	5.4255	5.42500
Least heat . . .	5.4258	5.4262	5.4235	5.42516
Original glass . .	5.4247	5.4261		5.42540

72. Here, notwithstanding the irregularities between the similar experiments, there seems, from the comparison of the mean specific gravities, to be a gradual though minute diminution of density, as the glasses have been more powerfully heated; and I found also, that when glass was so well stirred as to leave no doubt that it was thoroughly well mixed, yet being left in the furnace at a high temperature for eight or nine hours, it contained striæ.

73. On the other hand, first to render the glass perfectly free from bubbles and clear, and then to stir out the irregularities of composition, I have not

found to be a practicable process; because the stirring, in the manner in which I have yet performed it, tends to introduce bubbles into the glass; and though these are only small, still they are objectionable. Hence a mixed process has been adopted, which, as I have before stated, is subject to correction from future experiments. To render the process as far as it has been carried sufficiently intelligible to others, I will first describe the circumstances connected with stirring, and their influence upon striæ; and afterwards, the plans adopted for the dispersion of bubbles.

74. It is not a small degree of stirring and agitation which is sufficient to make a fluid of mixed materials homogeneous; especially when the mixture is not exceedingly fluid, but has a considerable degree of tenacity, something like tar or syrup. An idea of the extent to which it must be carried, and of the general nature of striæ in fluids, may be gained by taking a glass full of clear saturated syrup, made from white sugar, putting a few drops of water into it, and stirring the whole together. It may then be remarked how slow the striæ are in disappearing; and when they are apparently destroyed, if the whole be left for some hours, it will frequently happen that a separation will take place into a lower heavy, and superincumbent light portion, which when stirred together again produce striæ. In the glass, the stirring must be in the utmost degree perfect, for if there be the least difference in different parts, it is liable to form striæ: nor are the different portions allowed to arrange themselves by their specific gravities, in which case one part might perhaps be removed from another, after the glass was finished and cold; but the ascending and descending currents which inevitably take place in the fluid matter, are certain to arrange the irregularities in such a manner as to produce the strongest possible bad effect.

75. The instrument used for stirring has hitherto consisted of a piece of plate platina, which for the seven-inch glass, taken as illustrating the process, is $6\frac{1}{4}$ inches in length and $\frac{3}{4}$ths of an inch in breadth. It is perforated with various irregular holes, that, when drawn through the glass like a rake, it may effectually mix the parts. A piece of thick platina wire, about thirteen inches long, is riveted to it, and the extremity of this screwed into the end of a clean iron rod which answers the purpose of a handle. No small or cellular apertures should be allowed in this stirrer; for they will frequently retain air or moisture,

which may cause bubbles in the heated matter and do much harm. A little gold, therefore, should be applied to the part where the stem is attached, and fused, so that all hollows may be filled up. Stirrers of different dimensions are to be provided for different sized plates of glass. Before being used, they should be steeped in dilute nitric acid, and also heated to redness in the spirit lamp, just previous to their immersion in the glass for the first time in each experiment.

76. When a stirring is to be performed, the tiles and iron covers are removed from the first part of the chamber (44. 49. 65), the glass covers also taken off and put into the back part of the chamber (61. 65), the glass quickly examined, to give assurance that all is in good condition, and then the stirring commenced. The stirrer should be put in gently, that no air may be carried down with it, and then drawn through the glass quickly but steadily, so as to mingle effectually, but not to endanger forcing the substance over the edges of the tray or to run the risk of involving air bubbles. The chamber and its contents are cooled by the necessary exposure to the atmosphere, and therefore, when the agitation has been continued until the glass is so much lowered in temperature as to become thick, it should be discontinued, the stirrer carefully removed, the glass covers replaced, the chamber covers restored to their situation, and the temperature allowed to rise for fifteen or twenty minutes, when the operation may be renewed.

77. All the precautions against loose particles, dust, and soot, that were before spoken of (66), should be adopted in this operation. In the act of stirring, the instrument should not be struck carelessly against the bottom or sides of the tray; for the platina in this highly heated state is very soft, and a hole would readily be forced through it; nor should it be brought forcibly against the corners, for the metal is in such a favourable condition for welding, that the least blow upon a doubled part causes adhesion. By merely allowing the stirrer, when ignited, to sink upon the bottom of the tray rather more hastily than usual, it has adhered to the place; and when, for safety, an underlying plate of platina was used (50), it was always found welded to the tray at the places which the stirrer had touched a little more forcibly than the adjacent parts, and could not afterwards be separated without leaving holes in

the metal. This circumstance was the principal occasion of the advantages afforded by the use of the underlying plate being given up.

78. The heat which has to be borne during the operation of stirring, is very considerable, especially upon the hands; but at such a moment no retreat from the work, because of mere personal inconvenience, can be allowed. But the circumstance renders the use of a cover for the stirring hand very advantageous. I have found a loose linen bag, into which the hand could go freely, more convenient for this purpose than a glove; for being in contact with the skin at distant parts only, the hand is preserved at a much lower temperature. Two small holes in it, one at the front and the other at the top, allow the handle of the stirrer to pass obliquely through, by which arrangement it is easily held with firmness, and the bag itself prevented from slipping towards the glass. It should not be larger than to cover the wrist, or it will embarrass the movements; and it should be very stiffly starched, and ironed, that no fibrous particles may fly from it to the glass during the stirring.

79. The glass which, adhering to, is brought away with the stirrer, indicates, by its appearance, the general character and state of that in the tray; but during its examination, the experimenter must carefully refrain from touching it; for if the finger, or any other ordinary organic substance, come into contact with it, the next time the instrument is immersed in the ignited glass, the part touched will produce bubbles. It is therefore of importance that the stirrer be preserved perfectly clean from one stirring to another, for which purpose it may be deposited so that the platina shall be received in an evaporating basin, the mouth of which is afterwards covered over.

80. In entering upon the considerations relative to the bubbles, it will be evident, from the nature of the materials and the quantity of elastic matter originally present, that these are at first very numerous. The larger ones soon ascend to the surface, and breaking, are dissipated without inconvenience; but the smaller ones rise with far less readiness, and the smallest have so little power of elevation, that the general currents in the liquid appear sufficient to carry them downwards, or in any other direction, and thus retain them for any period within the mass. A useful idea of the length of time required for very minute bubbles to ascend through a fluid having some tena-

city, may be gained by the person who will take a glassful of clear concentrated white sugar syrup, and beat it up with a little air, until a portion of the latter is in extremely minute bubbles. If these are allowed to remain undisturbed, it will be observed, that though the larger bubbles rise quickly, and the smaller soon after, the smallest will continue for many hours under the surface, destroying the pellucidness of the fluid; and this will be the case although there are none of those descending currents, resulting from difference of temperature, which in the glass assist in retaining the bubbles beneath the surface.

81. From the great length of time which it required to liberate the bubbles even from small pieces of glass, and when no stirring was practised, I was induced to conclude that the evolution of gaseous or vaporous matter had not ceased upon the first fusion of the materials, but that the glass itself when highly heated continued to evolve small portions for some time. It occurred to me also, that in that case its formation might be hastened and the final separation advanced by mixing some extraneous and insoluble substance with the glass, to act as a nucleus, just as pieces of wood, or paper, or grains of sand, operate when introduced into soda water or sparkling champaign; in which cases they cause the gas, which has a tendency to separate from the fluid, to leave it far more quickly and perfectly than if they had not been present.

82. The substance I resorted to for this purpose was platina in the spongy state. It was chosen as being a body solid at high temperatures, uninfluenced by the glass, easily reduced to powder, and likely to retain its finely divided condition during the operation:—its preparation is described in the Appendix. In experiments made expressly to ascertain its action, it was found to assist powerfully in the evolution and separation of the bubbles, and afterwards to sink so completely to the bottom, that not a particle remained suspended in the mass. Even stirring does not render it injurious; for the particles, by that action, are welded to the bottom, and the glass ultimately equally free from mixture with them.

83. The spongy metal should be perfectly pure. It is easily reduced to powder by rubbing it with a clean finger on clean paper. No attrition with a hard substance should be allowed, as that burnishes the metal, and takes away the roughness, which is highly advantageous in assisting the evolu-

tion of the bubbles. When reduced to powder, it should be again heated upon a piece of platina foil in the flame of a spirit lamp.

84. The quantity of powdered platina which I have usually employed has been about 7 or 8 grains for every pound weight of glass. But in order to effect its more general and perfect diffusion, I have usually mixed it with ten or twelve times its bulk of pulverized glass. For this purpose, some of the rough glass, the same in composition with that to be perfected, has been crushed small in a clean agate mortar, and the finer parts separated from the coarser on an inclined and shaken sheet of paper. The former have been then mixed little by little with the platina, and rubbed slightly with the finger, to effect perfect separation of the metal, and then the coarser parts have been added, to increase the bulk. In this state it was ready for use.

85. The time of introducing this prepared platina is, like the times of stirring, as yet under investigation. It has usually been sprinkled from the platina ladle (28) over the surface of the well-fused and highly-heated glass, at the period of the first stirring. This method has the advantage of bringing the assisting substance into contact with the glass when the latter is highly disposed to throw off its adhering gaseous matter, and also allows of thorough mixture; but it also causes the addition of fresh glass after the concoction of the materials has been proceeding for many hours; and it likewise occasions the introduction of many bubbles formed by the air in the interstices of the powder.

86. On other occasions the prepared mixture of platina and glass has been introduced into the tray at the period when it was charged with the due quantity of rough glass, and before the application of fire. Particular attention was then paid to its general diffusion throughout the charge, and on these occasions its action commenced the moment the glass in contact with it was fluid. I am inclined to believe the latter will ultimately prove the better method of proceeding, both for the greater length of time during which the platina can act, and for the facility and convenience of its introduction.

87. In either mode of appliance the platina has been found highly serviceable; and in every case since its use, where stirring has not been necessary, the resulting glass has proved to be perfectly free from bubbles.

88. As already mentioned, the best periods for stirring and repose have not

been finally determined. Stirring introduces bubbles, and therefore should, if possible, be avoided towards the conclusion of the experiment. Rest, or at least that condition in which there is no other motion than what is due to the currents produced by slight differences of temperature, causes striæ even after very careful mixture (71. 72), and is therefore equally to be feared; and whatever other variations may have been adopted, I have always found it important to apply a careful concluding stirring. The following may be considered as the order of an experiment. If the spongy platina has not been introduced into the tray with the rough glass, then about the sixth hour after lighting the fire it is added in the manner already directed (85), and the glass well stirred (76). At about the twelfth hour the stirrings are recommenced, for the purpose of making the mixture perfect, and are repeated every 20 or 30 minutes, according to the fusibility of the glass and the state of the heat (60), for 8 or 9 times. The glass is then allowed to remain at rest for 6 or 8 hours, that bubbles may ascend and be dissipated, after which it is well stirred twice or thrice more with particular attention, that if possible no air may be introduced, and is finally mixed for the last time.

89. This concluding stirring is peculiar, in that it has to be continued until the glass is so cold and thick that no ascending and descending currents can be formed in it; after which its temperature is not again to be allowed to rise: hence the operation requires certain preliminary arrangements. The first point necessary is to clear out a considerable quantity of slag from the flue furnace, or that part beneath the chamber (47). This slag results from the fused ashes of all the coke which has been consumed there, with other portions that have passed on from the coal fire. It is to be drawn on to the bars of the furnace by a fire-rake which will pass into the passages beneath the chamber. If not taken out in its fused state, it would be impossible afterwards to remove it without risk of great injury to the furnace. At the same time that the slag is removed, all the coke is likewise to be withdrawn. All the fuel in the fire bars is also to be brought out of the furnace; and if the bars are embarrassed with clinkers, they are to be loosened. These things being done quickly and quietly, and the furnace apertures closed, a few moments are to be allowed for the little dust that may have been agitated to settle, and then the chamber

is to be opened and the glass stirred. The heat will have fallen very little by the preceding operations, and the glass may be well mixed, but with this precaution, that when once the stirrer is beneath the surface, it should not again be taken out until the conclusion. By opening the feed-hole, or the ash-pit, air may now be allowed freely to enter the furnace, and will rapidly lower its temperature, especially at such parts as the bottom of the pan, which are thin and at this moment exposed to the atmosphere on both surfaces. The temperature of the glass will fall in a corresponding degree, and the stirring being all this while continued, though more slowly if convenient, the substance will gradually thicken, until at last motion will endanger its being pushed out of the tray, and then the stirrer is to be carefully withdrawn. No currents in the glass need be feared, for the temperature cannot now rise higher. But a single cover being put over the tray, and the outer orifice of the air-tube closed by a good cork, the whole may be left a few minutes to cool still further for perfect security, until, the glass being supposed to have arrived at the state of a thick paste, the annealing should commence. Then the ash-pit, the fire-place, and all the other apertures to the furnace are to be closed; the second glass-cover put into its place; the chamber shut up by its iron and tile covers; a layer of bricks arranged close together over the whole upper surface of the chamber and furnace; the damper of the flue closed to prevent air passing through the fire-place, and the whole left to cool gradually for several days.

90. The interval between the common temperature and that at which the glass begins to lose solidity and acquire softness, is so much less with this variety than with flint glass, that it is probable a much shorter period of time is required for its perfect annealing than for the latter. That no failure might occur in this point, however, four days and nights have been allowed for the annealing of the large plates. If every thing were left as just described, the contents of the chamber would be warm on the sixth or even the seventh day, so gradually do the arrangements allow it to cool; but on the morning or the evening of the third day, according to circumstances, the damper in the flue is withdrawn a very little to allow the passage of a small quantity of air, and by this means the cooling facilitated and regulated.

91. When the furnace and its contents are cold, the chamber is opened: if

the experiment has been well conducted, every thing will be found loose, and unaltered in disposition from what they were when first arranged. The earthenware supports are to be removed, and the tray taken out. After examining the glass itself, the exterior of the tray should be carefully observed, whether there be any appearance of leakages either through imperceptible holes or at the corners, and such places as can be rectified by a patch should be noted in reference to the future use of the platina.

92. An operation which, to be successful, requires much care, is then to be performed; namely, the stripping off the platina from the glass. The tray should be placed on clean smooth paper upon a cloth. The corners are one by one to be opened by a blunt smooth knife, or some softer instrument, from the side towards which they were folded; and being then carefully pulled outwards by their extremities, will usually open, so that the platina becomes single again. Then proceeding from corner to corner, the platina will peel or strip easily from the sides of the glass, and will remain adhering by the bottom only. From time to time as fragments of glass are formed, they should be blown away or otherwise removed, that they may not cut the metal. If now the glass be placed a little over the edge of the table and firmly held, the platina may gradually be separated from the bottom in the same manner as from the sides, and the glass and the metal finally divided from each other without any injury to the former, and very little to the latter.

93. Immediately upon the separation of the platina, and before it can receive any mechanical injury beyond what it was impossible to avoid, it is to be put into a pickle consisting of nitric acid and water, and left there for several days. The dilute acid acts upon the adhering glass, dissolving and loosening it, and the plate is thus rendered fit for future operations (41). The stirrers also, when no longer required in an experiment, should be taken from their iron handles and put into the same pickling liquor. In this way the platina is perfectly cleaned, and being afterwards washed carefully in pure water and ignited, is again ready for use.

94. Such is the nature of the process as practised at present, by which plates of heavy optical glass seven inches square and eight pounds in weight have been prepared. I am encouraged to believe that it will admit of improvement, perhaps even to the full extent of our desires; but it will require time and patience

to effect it. As I have before said, we are in the course of our experiments only; and up to the last have seen reason to vary the arrangements, and still intend to make alterations. Every thing agrees to convince me that the size of the plate is not a circumstance involving any additional difficulty; but that, on the contrary, it will probably be safer to make a large than a small experiment. We can at pleasure obtain a glass perfectly free from striæ, unexceptionable in hardness, and with less colour than crown glass; but it is the simultaneous absence of all striæ and bubbles, with at the same time that degree of hardness and colour which will render the glass fit for optical purposes, that I am aiming at, and that I trust shortly to obtain.

95. As soon as the plates of glass are removed from the platina and briefly examined, they are sent to Mr. Dollond, who then enters upon the discharge of his particular duties in the Committee, by cutting, examining, and even working them into telescopes. It is not, however, my place to detail this gentleman's exertions (as a member of the Glass Subcommittee) in the cause of science. They will, I trust, appear in due season; and I hope that the want of perfect success on my part will not long be a cause of delay.

§ 2. *General qualities of the heavy Optical Glasses.*

96. A great variety of glasses have been formed by the use of different proportions of ingredients. They vary importantly from each other, though by no means to the extent of the difference existing between any of them and flint glass. The *specific gravity* rises very high in borate of lead, consisting of single proportions, i. e. nearly 24 by weight of boracic acid and 112 of oxide of lead; it is often as high as 6.39 or 6.4, being double that of some specimens of flint glass. In silicated borate of lead, which, in addition to the former quantities, contains 16 parts, or a proportional of silica, it is about 5.44. As the proportion of oxide of lead diminishes, so also does the specific gravity lessen, and it is in some of the specimens as low as 4.2; still permitting by the proportions present such fusibility and other qualities as consist with the process described. The specific gravity of Guinand's heavy flint glass is about 3.616; that of a specimen of ordinary flint glass 3.290; that of plate glass 2.5257; and that of crown glass 2.5448.

97. The *refractive* and *dispersive* powers of the glasses increase with their

specific gravity, as was to be expected. The powers of two of them, namely borate of lead, and silicated borate of lead, consisting always, if not otherwise expressed, of single proportionals, have been ascertained by Mr. HERSCHEL, and are as follows :—

	Bor. Lead.	Sil. Bor. Lead.
Angle of glass prism	29° 6′	30° 26′
Refractive index for extreme red rays . . . μ =	2.0430	1.8521
——— maximum yellow . . . — =	2.0652	1.8735
——— extreme violet :: . . . — =	2.1223	1.9135
Dispersive index = $\frac{\delta\mu}{\mu - 1}$ = ...	0.0740	0.0703

These intense powers upon light are not accompanied by any circumstance rendering the glass optically unfit for the compensation of the dispersive powers of crown or plate glass. Three object-glasses have been constructed for the express purpose of ascertaining this point; and all of them tend to demonstrate that the compensation or correction may be effected with equal if not greater facility than with flint glass.

98. One important circumstance connected with the application of these glasses to the purposes for which they are designed, is their *colour*. The great power they have of developing strong tints from metallic impurities, has been already described and illustrated (22. 23), and creates a difficulty in the way of obtaining them unobjectionably free from colour. The usual colour is more or less of yellow, and is perhaps almost altogether, if not quite, dependent upon the presence of a little iron. Like many of those dependent upon mineral substances, it is very much heightened by elevation, and lessened by diminution of temperature. It is rapidly and permanently diminished by increasing the proportions either of the silica or the boracic acid. The silicated borate of lead has latterly been obtained of such faint tint by the precautions relative to impurities, already described, that, when 9 inches in thickness, white paper looked at through it in open daylight resembled in appearance and depth of tint the surface of a lemon. Glass consisting of 1 proportional = 112 oxide of lead, 1 proportional = 16 silica, and $1\frac{1}{2}$ proportional = 36 boracic acid, when 7 inches in thickness and examined in the same manner, did not give a colour surpassing that of pale roll sulphur. The tri-borate of lead glass is almost as colourless as good flint glass, but might perhaps be found objectionable on other accounts.

99. As there is a certain quantity of light intercepted by glass which is altogether dependent upon and in proportion to its colour, it is evident that this property of the heavy glasses must be considered in relation to their use in telescopes; but there appears no reason for supposing they will ultimately prove inapplicable on this account. The colour of the glass already obtained is far less in depth than that of the crown glass constantly used in the construction of telescopes, which yet intercepts by its colour no important quantity of light; and if two plates 8 or 10 inches long, one of the yellow heavy glass and the other of crown glass, be looked through edgeways, it will be seen in a moment that the crown glass intercepts by far the most light. The colour of the glass is of no consequence, otherwise than as causing a loss of light from interception; for the tinge which is cast over objects looked at through a telescope constructed with it is scarcely perceptible to the most acute eye, and quite unimportant. When to these circumstances is added the reasonable expectation entertained of removing a large proportion of the little remaining colour by the use of purified silica (21), it will not be anticipated that experience will prove the glass faulty in this respect.

100. There is one very important action of the glass upon light, however, which may perhaps interfere more with its application, in telescopes at least, than any other, i. e. its *reflective power*. This is very strong in all the heavy glasses, far stronger than in flint, and exceedingly surpassing the similar power of crown glass. It is in proportion, as might have been expected, to the refractive power and the density of the glasses, all these properties increasing with the oxide of lead. The loss of light occasioned by the reflection from the two surfaces of a plate through which a ray is passed, appears to me to be greater than from the united action of both colour and bubbles in a piece of glass 7 inches thick.

I endeavoured to ascertain the comparative quantities of light reflected by these heavy and other glasses, in some photometrical experiments made upon the principle of similar shadows, measuring only the reflexion from the first surface of the different glasses, that from the second surface being destroyed. The ray was made incident in all the cases at an angle of 45°. It was obtained from a small single-wicked lamp *a*; and when reflected, its intensity was measured by the distance of a similar lamp *b*, whose direct light cast the

comparative shadow. The uniformity of the two lights, or at least of their relation to each other, was established by trials before and after the experiments with the reflecting surfaces, and each surface was tried two or three times, at intervals, and in a mixed manner; so that no anticipation of the result could in any case bias the mind. The following Table shows the results, small decimals being neglected:

		Inches.		
Light *a* direct		10.70	1	1
——— reflected by glass	5	36.75	11.80	$\frac{1}{11.8}$
	1	40.69	14.46	$\frac{1}{14.4}$
	4	43.46	16.50	$\frac{1}{16.5}$
	9	47.31	19.56	$\frac{1}{19.5}$
	6	50.31	22.12	$\frac{1}{22.1}$
	7	51.63	23.29	$\frac{1}{23.3}$
	3	52.69	24.26	$\frac{1}{24.3}$
	8	54.33	25.80	$\frac{1}{25.8}$
	2	54.56	26.02	$\frac{1}{26}$

The first column refers to the glasses below; the second gives the distance of the measuring flame *b*; the third, the preceding numbers squared and reduced to the direct light as unity; and the fourth, consequently, the proportion of the light *a* reflected by the first surface of each glass. No. 5 was glass consisting of 1 proportional of oxide of lead, $\frac{1}{2}$ a proportional of silica, and $1\frac{1}{2}$ proportional boracic acid. No. 1 was composed of 1 oxide of lead, 1 silica, and $1\frac{1}{2}$ boracic acid. No. 4, of 1 oxide of lead, $1\frac{1}{2}$ silica, and $1\frac{1}{2}$ boracic acid. No. 9 was flint glass; No. 6, 7 and 3, different pieces of crown glass; and No. 8 and 2, different pieces of plate glass. 1, 3, 5, 6 and 7, were natural surfaces; 2, 4, 8 and 9, polished surfaces.

The deficiency of light resulting from the increased reflecting power, though considerable, may easily be compensated for by slightly increasing the area of the plate; and the power of obtaining plates of any size is professed to be given by the general process: but whether that expedient involves any other objections, it will be for the optician to determine.

101. In *hardness*, these glasses differ from each other as much as in any other

quality, and indeed more. The borate of lead is very soft; the bi-borate of lead is harder, and the tri-borate equal to flint glass in hardness. The silicated borate of lead is softer than flint glass; but the glass consisting of 1 proportional oxide of lead, 1 of silica, and 1½ proportional of boracic acid, is as hard as ordinary flint glass, at the same time that it has that degree of fusibility, colour, and other properties, which makes it a very promising variety.

102. The hardness increases with the diminution of the oxide of lead; but the *fusibility* diminishes in the same proportion; and this is a property which it is essential to preserve to a certain degree for the removal of striæ and bubbles. The borate of lead is so fusible as to soften and lose its form under the surface of boiling oil. The silicated borate, and the glass consisting of the proportions above mentioned, are quite fusible enough to allow of the processes necessary for the removal of striæ and bubbles.

103. The fusibility of these glasses, and of glass generally, must not be confounded with their relative tendency to soften by elevation of temperature. It is not that glass which softens first, that becomes most fluid at a certain given high temperature; for glasses, like other substances, vary in their readiness to pass into the fluid state. Hence it has often occurred amongst the variety of compositions tried for glasses, that when the resulting substances have been placed side by side on platina foil, and heated, that which first softened did not when heated highly become so fluid as some other specimens that longer resisted the first impression of heat. It has however always been found that those glasses which when subjected to a rising temperature, most slowly passed from the solid to the fluid state, were also those which when subjected to long annealing processes, were least liable to assume a crystalline structure; and thus very useful indications of the probable qualities of compounds under investigation were often obtained.

104. A most important consideration relative to the application of these glasses to the construction of telescopes, is their liability to change and injury by the action of substances usually occurring in an ordinary atmosphere. When the value of a good object-glass is considered, frequently amounting to many hundred pounds, this point will be thought of no little consequence; and when it is known that even flint and plate glass are frequently injured in this way,

a little anxiety for the capability of resistance in the heavy glasses may readily be allowed, since they contain so much less of the substance (silica) which confers the power of resistance, and so much more of that (oxide of lead) which is considered as the vulnerable part, than does either of the former kinds.

105. The superficial changes of glass which interfere with its optical uses are of two kinds. The one is shown by a tarnish upon the surface, which when strong is iridescent. It is quickly produced by the intentional presence of sulphuretted hydrogen, which acting upon the oxide of lead present, reduces it, and forms a sulphuret of lead. It takes place only with flint glass, and is in every case produced either by sulphuretted hydrogen or other sulphuretted vapours. In plate glass the change is of another kind, and is shown by the appearance of minute vegetations or crystallizations, which spread, obstructing the light wherever they occur. Mr. Dollond, who has shown me cases of both kinds of injury in flint and plate glass, is inclined to believe that the latter has, during his long experience, proved most injurious.

106. From the commencement of the experiments it was expected that these heavy optical glasses would tarnish more than flint glass; but as specimens of borate of lead and other dense compounds of that metal had been retained in an ordinary atmosphere, without any particular precautions, for long periods of time, yet without tarnishing, there was encouragement to continue the investigations: and though when specimens were put into atmospheres purposely contaminated with sulphuretted hydrogen, they tarnished quickly, and much more than any flint glass, yet it did not follow that they should of necessity tarnish in the telescope; especially as, being from the construction of the achromatic object-glass inclosed by the tube and the crown or plate glass lens, they would be considerably protected, and at the same time would admit of the intentional application of extraneous chemical protectors.

107. The kind of protection which occurs to the mind is the application of such substances to the interior of the tube as, having a strong attraction for sulphuretted vapours, should continually retain the atmosphere within free from their presence. Carbonate of lead, precipitated borate of lead or finely-ground litharge, mixed with the pigment which is usually applied to blacken the inside of the telescope that all extraneous light may be absorbed, will probably effect this purpose completely.

108. A very curious and important influence of alkali in facilitating the tarnish of glasses containing oxide of lead, was discovered during the course of these investigations; and when the quantity of lead in flint glass is increased but a little beyond the ordinary proportions, its effect is powerfully manifested. Ordinary flint glass consists of 33.28 oxide of lead, 51.93 silica, and 13.77 potassa; the rest of the substances present, being in very small quantity, may be disregarded. Here the oxide of lead is 33.28 hundredths of the whole; and if it be only a little increased, for the purpose of giving greater dispersive power, the glass is liable to tarnish in an ordinary town atmosphere. Such is the case with a specimen of GUINAND's glass, which I have analysed, and which contains 43.05 oxide of lead, 44.3 silica, and 11.75 potassa. But provided the alkali be away, the quantity of oxide of lead may be enormously increased; and a glass containing 64 per cent of oxide of lead, in combination with 36 per cent of silica, has not tarnished by an exposure for 18 months on the same shelves with flint glasses that have tarnished. The following case will point out the effect still more strongly: A combination of equal weights of silica and oxide of lead was formed, and the compound has shown no tendency to tarnish in an ordinary atmosphere since February 1828. Eight parts of this was fused with as much pearlash as was equivalent to 1 part of potassa, and a glass was formed which has since become much tarnished. But other 8 parts being fused with 3 parts more of oxide of lead, so as almost to double the proportion of the latter, gave a glass without alkali, which does not yet exhibit the slightest trace of tarnish.

109. Hence the reason why the absence of alkali has been earnestly insisted upon in the preparation of the ingredients for the heavy optical glasses (18. 24). Hence the reason also why heavy flint glass, as already mentioned, has tarnished equally with some of the heavier glasses, though containing so much less lead, and of such inferior specific gravity. This influence of alkali is associated with, and perhaps directly referrible to, another circumstance affecting the liability of change in the glass; I mean the action of water or of aërial moisture, which is frequently considerable, and appears to be dependent upon the alkali present.

110. If a small quantity of flint glass be very finely pulverized in an agate mortar, then placed upon a piece of turmeric paper, and moistened with a drop

of pure water, strong indications of free alkali will be obtained. The same effect is produced by using plate glass; and if the pulverization be very perfect, the alkali can be detected in glasses containing far smaller quantities of that substance than either of those mentioned. This experiment, due to Mr. GRIFFITHS, shows that in whatever state of combination the alkali may be, it can still act upon, and is subject to, the action of moisture; and that flint glass is by no means a compound resulting from very strong chemical affinities, is also shown by an experiment which I made many years ago; namely, that if flint glass be pulverised exceedingly fine, the powder will indicate the presence of sulphuretted hydrogen in air by becoming blackened, almost as readily as carbonate of lead. Glass may be considered rather as a solution of different substances one in another, than as a strong chemical compound; and it owes its power of resisting agents generally to its perfectly compact state, and the existence of an insoluble and unchangeable film of silica or highly silicated matter upon its surface.

111. The half-combined and hygrometric state of the alkali appears to be the cause of the deposited film of moisture which is well known to adhere to ordinary glass when exposed to the atmosphere at common temperatures. This film is highly calculated to condense any portion of sulphuretted vapours which may be floating in the atmosphere, and thus bring them into contact with the oxide of lead under the most favourable conditions for the production of that action which is the direct cause of tarnish. Now from this cause of action the heavy glass is free; and hence a satisfactory reason to me why the heavy glasses have suffered so little when left with common care in an usual atmosphere.

112. An extraordinary difference exists between the electrical relations of this glass and other glasses, due principally to the same absence of alkali. Ordinary glasses, either flint, plate or crown, will, from the hygrometric film of moisture upon the surface, freely conduct electricity under common circumstances. Thus if a gold-leaf electrometer be diverged, and then touched with them in their ordinary state, the electricity is instantly discharged, even though the hand be two or three feet from the part touching the instrument. If a similar experiment be made with these heavy glasses, they have no sensible power of discharging the electricity, but insulate as perfectly as sealing-wax

or gum lac. If one of these plates of glass, without any previous warming and drying, be lightly brushed or wiped with flannel or silk, it instantly becomes strongly electrical, and retains its electricity for a long time; but it would be almost impossible to develop electricity by such slight means with flint or plate or even crown glass in a similar state. Hence the glass makes as good an electrophorus as lac or resin, and may probably be found hereafter to answer many useful electrical purposes. But the great point at present in view, is the proof which such electrical properties give of the absence of that film of moisture which is so constant upon other glasses.

113. All these circumstances are favourable to the opinion that the heavy glass will not be found objectionable in the construction of telescopes, because of any undue tendency to tarnish, and especially when precautions are taken to protect it from sulphuretted vapours in the manner before described (107). No difficulty can be anticipated in preserving the air within a limited and inclosed space free from such a contamination: to preserve it dry, if that had been necessary, under the different circumstances of varying temperature and the inevitable change of the air more or less frequently, would have been a far more difficult task.

114. The other kind of superficial change, i. e. the corrosion or crystallization which takes place principally on plate glass, is doubtless also due to the alkali present. Sometimes, indeed, specimens of glass may be found where the alkali being too abundant, a similar but more extensive action has taken place over the whole of the surface, and the glass falls off in scales. Whether the alteration be due to the action of the alkali on the water only, or on the carbonic acid and other substances it finds in the air, or to its united action on all together, is of little consequence at present, as the substance on which it depends is altogether absent from the glass under consideration.

115. Among the great number of glasses made, there are several of different composition, which have been selected, because of their general characters and properties, for more extensive trial and investigation when time will permit. Of these it would be useless to speak at present, as what might be stated of them now would probably require correction from future experiments. Up to this period the attention has been devoted, as it still must be for a while, to the establishment of a process which, competent to produce with certainty a

Fig. 1.

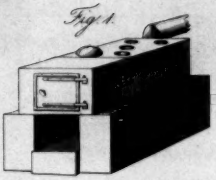


Fig. 5.

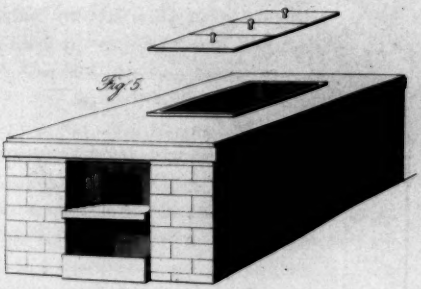


Fig. 2.

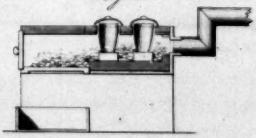


Fig. 7.

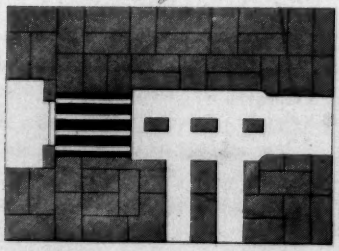


Fig. 3.

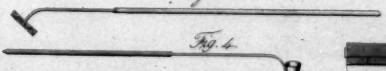
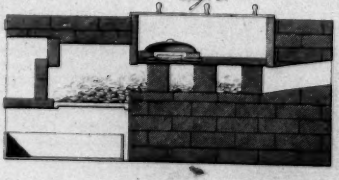


Fig. 4.

Fig. 6.



Scale.
Fig. 1. 2. 5. 6. 7. half an inch to a Foot.
3. 4. one inch to a Foot.

glass fitted for optical purposes, may have the philosophy and practice of every part so fully ascertained, as to be capable of description in a manner sufficiently clear to enable any other person, with moderate care, to obtain the same results without the labour of long and tedious investigation.

Appendix.

Rough glass furnace.

The only furnace for making rough glass which has been constructed, answers its purpose exceedingly well; and though if a second were to be made, it should be upon a larger scale, yet I think it better to describe the tried one accurately, than to direct alterations which have not been experimentally approved of; especially as there seems to be nothing which, in principle, need differ in a larger furnace. An iron box (see fig. 1. & 2.) 30 inches long, 14 inches wide, and $8\frac{1}{2}$ inches deep, forms the principal part of the exterior: it is open entirely at the top, and at the bottom also, in the fore part, where a fire-grate is to be placed. It has a common iron furnace door in front, the aperture of which is 8 inches wide by 6 inches high; and at the opposite end, or back of the furnace, a flanched aperture $6\frac{1}{2}$ inches by $4\frac{1}{2}$ for a piece of funnel pipe to connect the furnace with a powerful flue. The sides of this box, and such part of the bottom as is not appropriated for the fire-grate, are lined with fire-stone $1\frac{1}{2}$ inch in thickness, except in the fire-place, where it is $2\frac{5}{8}$. The grate is 12 inches long by 8 wide; and the part above it is closed by a fire-tile 2 inches thick and 12 inches square, which, resting on the edges of the lining, finishes the portion intended for the coal fire, leaving it $5\frac{1}{2}$ inches in depth from the covering tile to the grate. The other part is covered by an iron plate $17\frac{1}{2}$ inches long, 13 inches wide, and $\frac{3}{8}$ths of an inch thick, which, resting upon the edges of the lining, incloses a space of 16 inches long, 10 inches wide, and 5 inches deep, for the reception of crucibles. This plate is formed with circular holes about 3 inches, or rather more, in diameter, arranged as in the engraving, that

the crucibles inserted through them may leave plenty of room for the intervention of coke and flame. As many round crucible covers belong to the plate as there are holes, serving to close such of them as are not occupied by crucibles.

As the plate becomes very hot when in use, it is necessary to have a second above it, which may be formed of sheet-iron with corresponding holes, and when put into its place, separated from the first, a little space, by pieces of tobacco pipe, or other convenient substance, to include a layer of air. But it is much better for the retention of heat, and also for its superior cleanliness, that this second plate should consist of pieces of earthenware fitted to each other, so as to cover the surface of the iron plate, from which it should also be separated by a short interval.

The crucibles used are 5 inches high outside, $3\frac{1}{2}$ inches diameter at the top, and 2 inches diameter at the bottom. They are of pure porcelain biscuit, perfectly white and clean. They should be made as thin as possible, of the finest and most refractory kind of ware, and baked at a high temperature. We have some crucibles made about thirty years ago for Mr. Hatchett, which, though not of the size required, are precisely the right kind of ware. They have been used many times in succession without cracking or being importantly acted upon by the glass, and no sensible degree of impurity was given to it from them.

When these crucibles are arranged in the furnace, they should be supported by little stands of earthenware, formed out of brick or Cornish tile, so that their edges shall rise about the $\frac{1}{2}$ or $\frac{1}{3}$rd of an inch above the surface of the upper covering plate, that no impurity may enter them. The holes in the plate should be of such dimensions that, when hot, the crucibles may fit loosely, that they may be uninjured, and also that there may be room between for the vapours that are evolved from the mixture to pass away.

The covers to the crucibles are evaporating basins about $4\frac{1}{2}$ inches in diameter. They are slung with their edges downwards by a piece of platina wire sufficiently strong for the purpose, which being first bent at the middle into an angle, is then stretched across the outside of the basin, and has its ends bent round the opposite edges of the latter. The bent extremity of an iron rod passed under the loop thus formed over the middle of the bottom, serves to

raise and remove the cover from place to place. When a crucible is in use, the cover should be arranged over it in such a manner as not to touch the vessel, but rest by its edges on the earthenware plate around.

The platina stirrers in use with this furnace have been before described (28. 75) fig. 3. The platina ladle consists of a small crucible of that metal riveted to a platina wire, and that made fast by a screw to an iron rod. (fig. 4.)

The use and manner of working this furnace will be well understood from the above description, and what has before been said (26, &c.). The crucible should never be suddenly heated or cooled. The coke may be fed and arranged at such of the crucible holes as are out of use at the time. From the very valuable effects of a trough of water under the fire bars (45) experienced in the larger furnace, one is constantly used in that just described.

Finishing furnace.

This furnace on the outside is a parallelopiped, principally of brickwork, built against a wall, 64 inches in length from the fire front to the beginning of the flue, against which it is built, 45 inches wide, and 28 inches high. (fig. 5. 6. & 7.) It is the only one that has yet been built, and, for the reasons before given, shall be described exactly as it is. The fire-place is at one end, and the course of the flame and smoke is directly from that to the other end, and then immediately into the upright flue. The fire-place is 15 inches from back to front, 13 inches wide, and $11\frac{1}{2}$ inches from the arched roof to the bars. Its outward side, or that from the wall, is $18\frac{1}{2}$ inches in thickness of brickwork, which is intended to give stability to the structure. The mouth of the fire-place is an aperture 8 inches by 6 inches, made in a piece of fire-stone 7 inches inwards from the front of the brickwork: its lower edge is level with a fire-stone sill, which, extending forwards from the fire-place to the outer surface of the brickwork, forms a shelf, on which two bricks stand, that serve in place of a door to close the mouth of the furnace. The ash-pit is 25 inches long, 12 inches wide under the fire, and 10 inches high to the bars. A trough made of rolled iron, riveted together, and $5\frac{1}{2}$ inches high on the sides, occupies its lower part. This being preserved full of water, is sustained at the boiling temperature by the radiation of heat and the hot ashes which fall into it.

From the back part of the fire-place, and 2 inches above the level of the fire-

bars, the brickwork is carried on horizontally until close to the stack. The sides of this part are perpendicular, and 12 inches apart: they are continued upwards to the top of the brickwork 14 inches unbroken, except that at 5 inches from the bottom they are thrown back $\frac{1}{3}$rd of an inch so as to form a ledge there. This ledge is for the purpose of receiving the edges of certain fire-tiles, which, when put in, form the top of the flue and at the same time the bottom of the glass chamber; but the whole is so constructed, that the tiles can be put in and taken out at pleasure without disturbing the rest of the work. The side or rather end of the chamber nearest the fire is constructed of a fire-tile, which terminates and faces the brick arch over the fire-place, and extends from the surface of the brickwork downwards 9 inches to the side ledges before described: the further end of the chamber is finished in a similar way, and beyond that the flue is carried in the most convenient and direct manner, but without any unnecessary contraction, into the stack or chimney. The length of this upper aperture, afterwards constituting the chamber, is 25 inches, its breadth 12$\frac{3}{4}$ inches. When the bottom tiles are in their places, they leave a depth of 5 inches for that part of the furnace or flue beneath the chamber, which is also 38 inches from the fire to the end, and, with the exception of certain supports in it, is 12 inches wide.

These supports are built in with the bottom of the flue. They are essential to the permanency and regularity of the bottom of the glass chamber, and require considerable nicety in their arrangement. They consist of fire-bricks placed up on end, so that their narrowest surfaces are towards the ends of the furnace, their sides or broadest exposed surfaces parallel with the sides of the furnace itself. They rise to the same height above the bottom of the flue as the ledges on the sides of the brickwork, or 5 inches, and with them, form the support for the bottom tiles. There are three of them in our furnace, placed in a line equidistant from the two sides of the flue; and being 2$\frac{1}{2}$ inches thick, they leave spaces for the passage of flame and the reception of coke, which are 4$\frac{3}{4}$ inches in width. The first of these is two inches from the back edge of the fire, and in that direction extends 4 inches; the second is 4 inches from the first; and 6 inches beyond that one is the third.

During the action of the furnace, coke is supplied to this part, and arranged through two holes level with this space, and wrought in the side of the furnace

by leaving out a brick. They are made to occur nearly opposite the spaces between the supports seen when looking across the course of the flame, and are stopped by the insertion of loose bricks, and a piece of paper put before the place, which adheres from the pressure inwards of the atmosphere. These holes, being in the thickness of the walls of the furnace, are 17 inches long.

The tiles which form the bottom of the chamber and top of the flue, are of Cornish ware (52. 53), or at least the one which constitutes the half nearest the fire is of that material; but the other, which is not so highly heated, and never has to be moved, may be some other ware, and $2\frac{1}{2}$ inches in thickness. The tile nearest the fire has to transmit heat to the glass, and if of Cornish ware, and supported as described, is abundantly strong when $\frac{3}{4}$ths of an inch in thickness. It should be nicely adjusted by grinding (53), and when fitted in, the edges should be made close by a little fire lute.

There is a part of the furnace not yet mentioned, which must be arranged as the structure is raised. This is the air tube (55). It is of glazed porcelain, and passes horizontally through the side of the furnace, so that its inner aperture is 2 inches from the end of the glass chamber, and its lower edge level with the upper surface of the Cornish tile constituting the bottom, whilst the outer end is flush with the outside surface of the brickwork. Its length is 17 inches, its internal diameter $\frac{7}{8}$ths of an inch. The short pieces of adjusting tube (55) are 6, 7, and 8 inches in length, and $\frac{1}{10}$ths of an inch internal diameter: their ends are usually finished obliquely.

All those parts of the furnace which are in contact with or near the fire, are of the best fire-bricks laid in loam; but the sides of that part of the cavity already described, which form the glass chamber, are fire-tiles, and they rise about an inch above the neighbouring brickwork, forming a raised edge all round, which, at the same time that it better excludes dirt than if level with the rest of the work, also allows the covers of the glass chamber to apply more closely. These covers are three wrought-iron plates, each $\frac{1}{4}$th of an inch in thickness and 16 inches long; but their widths vary, and are 7, 10, and 12 inches. These put side by side cover the mouth of the chamber, but varied in juxta-position, allow of more or less of the chamber being opened at once, according to whatever the experiment may require; each has a short solid handle fixed to the middle of the upper surface.

Besides the iron covers, there are a set of earthenware covers, consisting of 6 square tiles each $1\frac{3}{8}$ inch thick (62). These are notched to receive the handles of the iron covers, and being put together over them, constitute a covering of earthenware, which very importantly assists in retaining the heat.

The tiles and brick used in the annealing process (89) are the ordinary dry varieties, with some pieces of various sizes, to allow of the close adjustment of the whole.

The earthenware supporting blocks (57) required for the arrangement and support of the platina tray should be formed out of some kind of flat unglazed ware containing as little iron as possible, and should be of various thicknesses, sizes, and forms, although parallelopipeds are the most usual. They should not be of such substance as is liable to fly or send off anything when heated; and when any portion of glass adheres to them, it should either be cleared off or the piece thrown away. The Cornish tile before described (52. 53) is excellent for this use, and may be sawn, rasped or ground into any shape required.

The glass covers (60, &c.) that have yet been used were merely inverted evaporating basins. They answer the required purpose exceedingly well, except that, when large, they are too strong, too heavy, and too deep. Some covers for the purpose are therefore in progress, and as they only have to support their own weight and hold together, they are to be thin. The covers should be of very refractory and highly baked ware; it may be desirable to have them very slightly glazed, to keep them clean, and prevent the absorption of any substance which might send off vapours injurious to the glass.

The fire tools required for this furnace will suggest themselves. Amongst the rest should be a pair of tongs which will readily lay hold either of the earthenware tile or the iron covers; a slag and coke rake (89); and a stoking iron, with its extremity bent, for the purpose of breaking the clinkers off the bars from beneath upwards.

Preparation of spongy platina.

The platina used for this preparation should be pure, and may be the refuse pieces resulting from such plate and foil as has been in use for trays in former experiments. This, after being taken out of the pickle (93), and condemned as useless for other purposes in the glass-house, should be trimmed from all

alloyed parts, if any such are adhering to it, and then digested in a Florence flask, with a mixture of five measures of strong muriatic, one measure of strong nitric acid, and three measures of water. But little heat should be applied at first until the action diminishes. According to Dr. Wollaston, one ounce of platina will be dissolved by about four ounce measures of such acid, and it is advantageous to have a considerable excess of platina present. The solution obtained is to be precipitated by a strong solution of muriate of ammonia; a bright yellow pulverulent substance will fall, and a mother liquid having more or less colour remain. The precipitate being allowed to subside, the liquor is to be poured off, and the former then washed with two or three portions of water. The washing liquors and the mother water may afterwards be concentrated together; but it is better not to prepare spongy platina for this particular use from these fluids, but only from the precipitate which falls on adding the muriate of ammonia.

The yellow precipitate, when washed, is to be dried on a filter, or in a basin, and then decomposed by the application of a dull red heat. This may be done in a clean white earthenware crucible. The heat should be continued until vapours cease to arise; but this will be found a long operation, in consequence of the low temperature which is to be applied, and the exceeding bad conducting power of platina for heat when in this spongy state. The reduction may also be performed by putting the precipitate upon a piece of platina foil in a layer about $\frac{1}{4}$th of an inch in thickness, and covering it with another piece of foil; a spirit lamp will then suffice to reduce the metal, but the foil and powder must be turned occasionally, that both sides may be exposed to the flame. The platina will appear as a dull grey spongy metallic mass. It should be broken up, mingled, and then again heated to insure the dissipation of all volatile matter.

After this is done, the platina should be rubbed to powder by the clean finger, or clean paper (83), heated slightly a third time, and then preserved in a clean and well stoppered bottle.

II. *Account of Levellings carried across the Isthmus of Panamá, to ascertain the relative height of the Pacific Ocean at Panamá and of the Atlantic at the mouth of the river Chagres; accompanied by Geographical and Topographical Notices of the Isthmus. By* John Augustus Lloyd, *Esq. Communicated by* Captain Sabine, *Secretary of the Royal Society.*

Read November 26, 1829.

In November 1827 I received a special commission from General Bolivar to make a survey of the Isthmus of Panamá and Darien, in order to ascertain the best and most eligible line for a communication (whether by road or canal) between the two seas. On my arrival in Panamá in March 1828 I was joined by a brother officer of Engineers, a Swede in the Colombian service, a good mathematician and of habits of great correctness in observation.

Upon consulting together, we found that we could combine the particular object of the commission with a second object in which we both felt a deep interest, namely, the determination of the relative height of the ocean on either side of the Isthmus; and that we could best accomplish both, by taking a part of the present line of road between Porto Velo and Panamá, until we should fall in with the river Chagres about twenty miles above Cruces, which village is the usual landing-place for all articles of commerce in their transit from the North Sea to Panamá.

To avoid delay, we commenced our operations on the 5th of May, although the rainy season had for some days set in; being resolved to overlook the absence of personal comfort, the unhealthiness of the season to a European constitution, the inadequacy of our means, and various other difficulties unnecessary to enumerate, as we finally succeeded in surmounting them.

The instruments used for the levelling were,—A 20-inch spirit level of Carey's best construction, with extra telescopes, levels, shade tubes, &c., which I received from the museum at Bogota; a pair of excellent station staves made by Harris and graduated as usual, with vernier scales added by myself to read off to thousandths of a foot when required; Gunter's chains; an

excellent 10-inch theodolite by Carey; and a very fine altitude and azimuth circle for the survey.

Our first level commenced at the end of the street called Callé Sal si Puede in the suburbs of Panamá and at the point of a bay called Playa Prieta, at spring tide high-water mark, observed two days after full and change, which I had subsequently the opportunity of verifying on my return to Panamá, and of ascertaining that it was 3.03 feet lower than the extreme rise of occasional tides under the influence of particular winds. From this point we followed the old road to Porto Velo, and after 732 pairs of levellings in a distance from Panamá of 1828 chains (22⅔ miles), we arrived at the banks of the Chagres on the 30th of June, 633.32 feet being the greatest height we had passed over; and after building a secure station on the bank 169.84 above the level of high water mark in the Pacific, we finished our operations for that year, on account of the great inclemency of the season, the sickness and debility of the people employed, and from our own constitutions beginning to suffer by continued exposure to incessant rains, with generally no other covering than tents and ranchos or small huts built by ourselves.

On the 7th of February 1829, the dry season having for some time set in, we resumed our levelling at the station at which we had desisted the year before, having the instruments in good repair; and having descended to a station on the river fixed on for the purpose, we found the surface of the water to be 152.55 feet above high-water mark in Panamá.

We now followed the course of the river, and were enabled thereby to take longer levels than before, which were made with the greatest possible care, reducing the observations to the true levels by the most exact tables for the curvature of the earth.

After 68 pairs of levellings we arrived at Cruces in a distance of 1545 chains (19¼ miles), and found a fall in the river of 114.60 feet, leaving only 37.96 feet above the Pacific. Having nearly 50 miles more to descend, and finding so great a fall in 19 miles, we were led to expect a greater fall than 37.96 feet in the remaining distance; and consequently, at this stage of our operations, to apprehend that we should find the level of the sea at Panamá to be higher than at the mouth of the Chagres.

From Cruces to a town called Gorgona, distant 419 chains (5¼ miles), there is

a fall of only 16.13 feet, and thence to a small gravel bank named by us "Playa de los Injenieros," distant altogether 1302 chains (16¾ miles) from Cruces, we found a fall from Gorgona of 21.82 feet, to a station precisely level with the high-water mark of spring tides in the Pacific, still being 34 miles from the mouth of the river. From this point we descended below the level of high-water-mark of the Pacific, until we arrived at a place called Palo Matias, distant from Cruces 2682 chains (33½ miles), and from the commencement of the levels in the river 4227 chains (52½ miles): at this point we first observed the effects (slight as they were) of the tides of the North Sea, and the height of the water was 13.65 feet below the high-water mark at the Pacific; this we therefore concluded to be the level of high-water mark in the Atlantic. We however continued our levellings 507 chains further, to a place called La Bruja, nearly 12 miles distant from the mouth of the Chagres, where the water in the dry season is very brackish, and from whence there is no perceptible current to the sea: here we found the level of the surface of the water, by several observations at the time of high water in Chagres, to be 13.55 feet below the high-water mark in the Pacific, being 0.1 of a foot less than at Palo Matias, the difference being occasioned by our not having observed the tides at the former place so correctly as at the latter, as we determined to finish the observations at La Bruja. After 935 pairs of levellings, therefore, in a distance of nearly 82 miles, we found high-water mark in the Pacific to be13.55 feet higher than high-water mark at La Bruja, which, from the circumstances above mentioned, is considered to be the high-water level of the Atlantic at Chagres.

The details of all the operations connected with the levellings are contained in a manuscript deposited in the library of the Royal Society; they are further illustrated by a section of the whole on the scale of four inches to a mile.

No proof levels were taken. I was aware, from the commencement, of the nature of the task I had entered upon, and knew too well that if I had the good fortune to be able to persevere a sufficient time to carry the levelling across the Isthmus, I should be but little capable of remaining during a third year, which such verification would have required. I therefore adopted such a scrupulous and rigid mode of proceeding as would render a verification unnecessary, and prevent the intrusion of even a trifling error; an important one I am bold to say was nearly impossible. In the whole distance overland to the river Chagres, my companion being employed with the chain, I was

assisted by a Spaniard whom I had previously instructed in the management of the station staff. By means of signals I made him adjust the cross-piece correctly to the horizontal wire of the telescope; he then brought me the staff, which I read off and noted down the reading; he resumed his station, I examined the level again, adjusted the staff, recalled the Spaniard, and read off a second time.

From the Chagres to the last level my companion had the station staff himself, which when adjusted to the cross-wires was read off by him. I then re-examined the level, adjusted the staff a second time, and he again read off, writing down the two observations in distinct books, which were compared in the evening. The instrument itself I proved on most days after work, by making a set of 8 or 10 levellings in a circle; returning in the last station to the point from which I first started, and finding the sum of the differences of the levellings amount to zero.

The point from which the levellings commenced at Panamá is marked by a large stone cut for that purpose in the wall at the edge of the sea in Playa Prieta; and the concluding point at La Bruja, by a tree cut down to the exact height marked in the Observation Book above the surface of the water: the height of the section of the tree is 6.848 feet below the level of high-water spring tide at Panamá. No better means of marking this level presented itself at La Bruja; but by a reference to the manuscript detail of the observations preserved in the library, several stations are to be found in the vicinity which are not liable to decay.

By careful and continued observations, I found the rise and fall of the tide in the Pacific at Panamá as follows: between the extreme elevation and depression of the waters by occasional tides there is a difference of 27.44 feet, and the mean actual rise and fall two days after full moon is 21.22 feet.

At Chagres I observed the rise and fall of the tide at the close of the dry season in April 1829 to be 1.16 foot, and being there subsequently during the rainy season, I had an opportunity of observing that the high-water mark was the same in both seasons.

The time of high water is nearly the same at Chagres and at Panamá, namely at $3^h\ 20^m$ at full and change: hence the following interesting and curious phenomena are deducible in respect to the difference of level of the two seas.

First.—High-water mark at Panamá is 13.55 feet above the high-water mark

of the Atlantic at Chagres; half the rise and fall of spring tides is at Panamá 10.61 feet, and at Chagres 0.58 of a foot; and assuming half the rise and fall above the low water of spring tides to be the respective mean levels, the mean height of the Pacific at Panamá is 3.52 feet higher than that of the Atlantic at Chagres*.

Second.—At high water, the time of which is nearly the same on both sides the Isthmus, the Pacific is raised at mean tides 10.61 feet, and the Atlantic 0.58 of a foot, above their respective mean levels; the Pacific is therefore the highest at such times by (10.61−0.58+3.52=) 13.55 feet.

Third.—At low water, both seas are the same quantities below their respective mean levels; therefore at such times the Pacific is lower than the Atlantic by (10.61−0.58−3.52=) 6.51 feet.

In every twelve hours therefore, and commencing with high tides, the level of the Pacific is first several feet higher than that of the Atlantic; it becomes then of the same height, and at low tide is several feet lower: again, as the tide rises the two seas are of one height, and finally at high tide the Pacific is again the same number of feet above the Atlantic as at first.

Almost every person who visits Panamá from the Atlantic side is disposed to think that the country rises from the Atlantic to the Pacific. The ascent of the river Chagres, particularly when swollen by rains and its current rendered more than usually rapid, is very toilsome; and on reaching Cruces after a four or five days tedious journey, a traveller is impressed with the persuasion that he has gained a considerable elevation above the sea that he has quitted: this impression is not diminished by the journey to Panamá, which is mostly through rough and rugged passes, continually ascending and descending; and when, on arriving in the savannahs, a few miles from Panamá, the city is beheld for the first time with its conspicuous cathedral, the general exclamation is, "I thought Panamá had been near the level of the sea." Such is actually its situation; but as the valley from which it is first seen is several feet below the level of the sea, the first and strong impression produced, is that the city stands upon an eminence.

* The author is aware that there are different opinions with regard to what is the mean level of the ocean; the assumption in the text is conformable to his own opinion, but as the data are given from which the conclusions are drawn, every person is furnished with the means of making his own deductions.

Topographical and Geographical Notices.

In the map which accompanies this memoir, the coast line on both sides the Isthmus is taken from the best Spanish authorities, including some very recent corrections; the interior is wholly from my own observations.

The district which extends from Panamá along the old road to Porto Velo, as far as its meeting with the river Chagres, and for three to five miles on either side, is from a survey made by my companion Captain Falmarc and myself, whilst levelling through this part of the country, in the operations already described. The angles and bearings were taken with a 10-inch theodolite by Carey, with a needle as exact as could be provided for an instrument of that size. On a separate plan (in MSS. deposited in the Society's library,) are marked the stations of the survey, and the intersection of the bearings taken from them. The principal stations are as follows:— The Cerros or Mountains of Ancon, Caledonia, Vidrio, Lirio, Algarobo, Pelado, Largo, Gordo, San Sonati, Alto, de Las Lajas, Maria Henrique, Grenadilla, &c.

From these stations, the surrounding country was sketched at once on the rough map: the same mode was continued from the point where the levellings intersected the river Chagres to the mouth of the river, including as much of the surrounding country as could be laid down by intersecting bearings, taken on either side the river.

The country west of the road from Cruces to Panamá, including a few miles of the coast by Arayjan and Chorrera, was traversed in various directions with a compass, every accessible eminence was ascended, and views taken of the country.

The country between the northern banks of the river Chagres and the North Sea was examined and sketched in an excursion in which I ascended the river Gatun, with a boat compass, and crossed from thence on foot by the gold mines of Santa Rita, (where is gained a fine view of the northern coast to La Enseñada de las Minas,) and thence to the sea beach: from this point I pursued the coast line to Porto Velo, and recrossed the Isthmus to Panamá on foot, in the route marked in the map, which is the old and only road from Porto Velo to Panamá, taking careful bearings from eminences over which I passed,

and gaining as many views of the surrounding country as I possibly could, by climbing the highest trees with a small tomahawk, particularly noticing the direction and figure of the Cordillera to the east and west.

The more direct line from Porto Velo to Panamá, passing through the river Chagres at a place called Calle Limon, is laid down from a manuscript furnished by a Spaniard, who with a circumferentor and a cord of 200 varas crossed from Porto Velo to Panamá as nearly north and south as possible.

It is generally supposed in Europe that the great chain of mountains which in South America forms the Andes and in North America the Mexican and Rocky Mountains, continues nearly unbroken through the Isthmus. This however is not the case: the northern Cordillera breaks into detached mountains on the eastern side of the province of Veragua. These are of considerable height, extremely abrupt and rugged, and frequently exhibit an almost perpendicular face of bare rock. To these succeed numerous conical mountains rising out of savannahs and plains, and seldom exceeding from 300 to 500 feet in height. Finally, between Chagres on the Atlantic side, and Chorrera on the Pacific side, the conical mountains are not so numerous, having plains of great extent interspersed, with occasional insulated ranges of hills of inconderable height and extent. From this description it will be seen that the spot where the continent of America is reduced to nearly its narrowest limits, is also distinguished by a break for a few miles of the great chain of mountains, which otherwise extends, with but few exceptions, to its extreme northern and southern limits.

This combination of circumstances points out the peculiar fitness of the Isthmus of Panamá for the establishment of a communication across.

On the east of the line from Panamá to the Bay of Limon, the mountains again commence, gradually thicken, and become more elevated until they connect and form Cordilleras extending from Porto Velo to the Bay of Mandinga, from whence there is another break in the province of Darien and Choco, after which the land rises into a Cordillera on a very extended scale and of very great elevation.

Two lines are marked on the map, commencing at a point near the junction of the rivers Chagres and Trinidad, and crossing the plains, the one to Chorrera and the other to Panamá. These lines indicate the directions which I consider

the best for a rail-road communication. The principal difficulty in the establishment of such communication would arise from the number of rivulets to be crossed, which, though dry in summer, become considerable streams in the rainy season.

The line which crosses to Chorrera is much the shortest, but the other line has the advantage of terminating in the city and harbour of Panamá.

The country intersected by these lines is by no means so abundant in woods as in other parts, but has fine savannahs, and throughout the whole distance, as well as on each bank of the river Trinidad or Capira, presents flat and sometimes swampy country, with occasional detached sugar-loaf mountains, interspersed with streams that mostly empty themselves into the Chagres.

Should a time arrive when a project of a water communication across the Isthmus may be entertained, the river Trinidad will probably appear the most favourable route. The river is for some distance both broad and deep. Its banks are also well suited for wharfs, especially in the neighbourhood of the spot from whence the lines marked for rail-road communications commence.

As the river Chagres has been greatly dwelt on in the writings of those who have discussed the probability of communications being established between the two seas, and as considerable expectations have been formed of the facilities it might afford towards a water communication, I have given a separate plan of the river from its mouth to the point at which it was intersected by the levellings. (This plan will not admit of reduction within the compass of the plates in the Philosophical Transactions, but will remain in the Society's library, where it may be consulted. A plan of the river on a less minute scale is contained in the general map.)

The distances along the river were measured by a strong line of 10 chains in length, substituted for the usual measuring chain, properly subdivided, and its length occasionally verified; the cord was borne usually by five men, but when required in the shallow and rapid water, by as many as ten men; and when the water became too deep for the men to wade, canoes were employed to stretch the line. The soundings were taken by a man with a sounding line marked to half feet, seated behind me in the canoe; so that I could observe the line myself at every cast. The casts were generally between 30 and 40 yards

from one another, and the depth of water was inserted in a sketch of the river which I was at the same time engaged in making.

The river, its channel, and the banks, which in the dry season embarrass its navigation, are laid down in the manuscript plan with great care and minuteness. It is subject to one great inconvenience, that vessels drawing more than 12 feet water cannot enter the river, even in perfectly calm weather, on account of a stratum of slaty limestone, which runs, at a depth at high water of 15 feet, from a point on the main land to some rocks in the midle of the entrance of the harbour, and which are just even with the water's edge; which, together with the lee current that sets on the southern shore, particularly in the rainy season, renders the entrance extremely difficult and dangerous. The accompanying plan of the harbour will sufficiently explain the inconveniencies it is liable to.

The value of the Chagres considered as the port of entrance for all communications, whether by the river Chagres, Trinidad, or by rail-roads across the plains, is greatly limited from the above-mentioned cause. It would prove in all cases a serious disqualification, were it not one which admits of a simple and effectual remedy, arising from the proximity of the Bay of Limon, otherwise called Navy Bay, with which the river might easily be connected: the coves of this bay afford excellent and secure anchorage in its present state, and the whole harbour is capable of being rendered, by obvious and not very expensive means, one of the most commodious and safe harbours in the world.

By the good offices of H. M. consul in Panamá, and the kindness of the commander of H.M. ship Victor, I obtained the use of that ship and her boats in making the accompanying plan of this bay. The shores are laid down trigonometrically from a base of 5220 yards, the situation of which is marked; and the soundings were taken by myself with the assistance of the master. It will be seen from this plan that the distance from one of the best coves (in respect to anchorage) across the separating country from the Chagres, and in the most convenient track, is something less than three miles to a point in the river about three miles from its mouth.

I have traversed the intervening land, which is particularly level, and in all respects suitable for a canal, which, being required for so short a distance, might

well be of sufficient depth to admit vessels of any reasonable draft of water, and would obviate the inconvenience of the shallow water at the entrance of the Chagres.

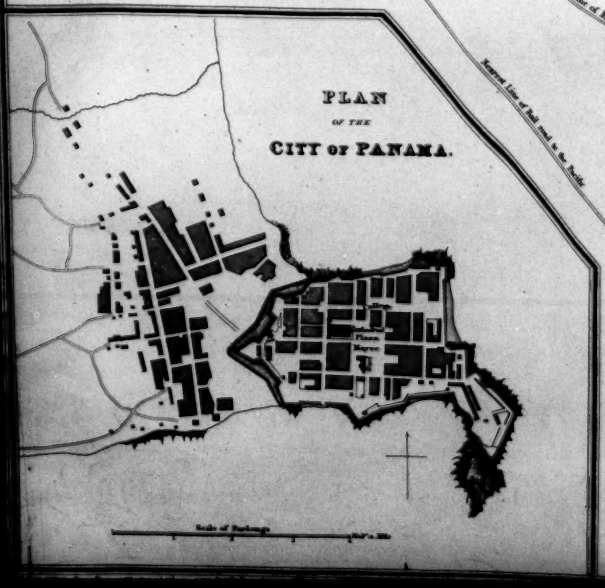
I have felt that I might be expected to state my own opinion of the mode that offers the greatest facility for communications across the Isthmus, since my examination and surveys were made for that specific purpose, and I have accordingly done so; but I have endeavoured at the same time to render the topographical representation of the country sufficiently detailed to enable others to draw such conclusions as may perhaps deserve to be preferred to mine, with almost as much advantage as if they had themselves visited the country.

For the opportunities that I have thus enjoyed of contributing correct topographical knowledge of a part of the world, which from its peculiar locality has attracted much philosophical and commercial interest, I am indebted to the authority and support which I received from General Bolivar; I am also indebted to his liberality for permission to make public the information I have acquired.

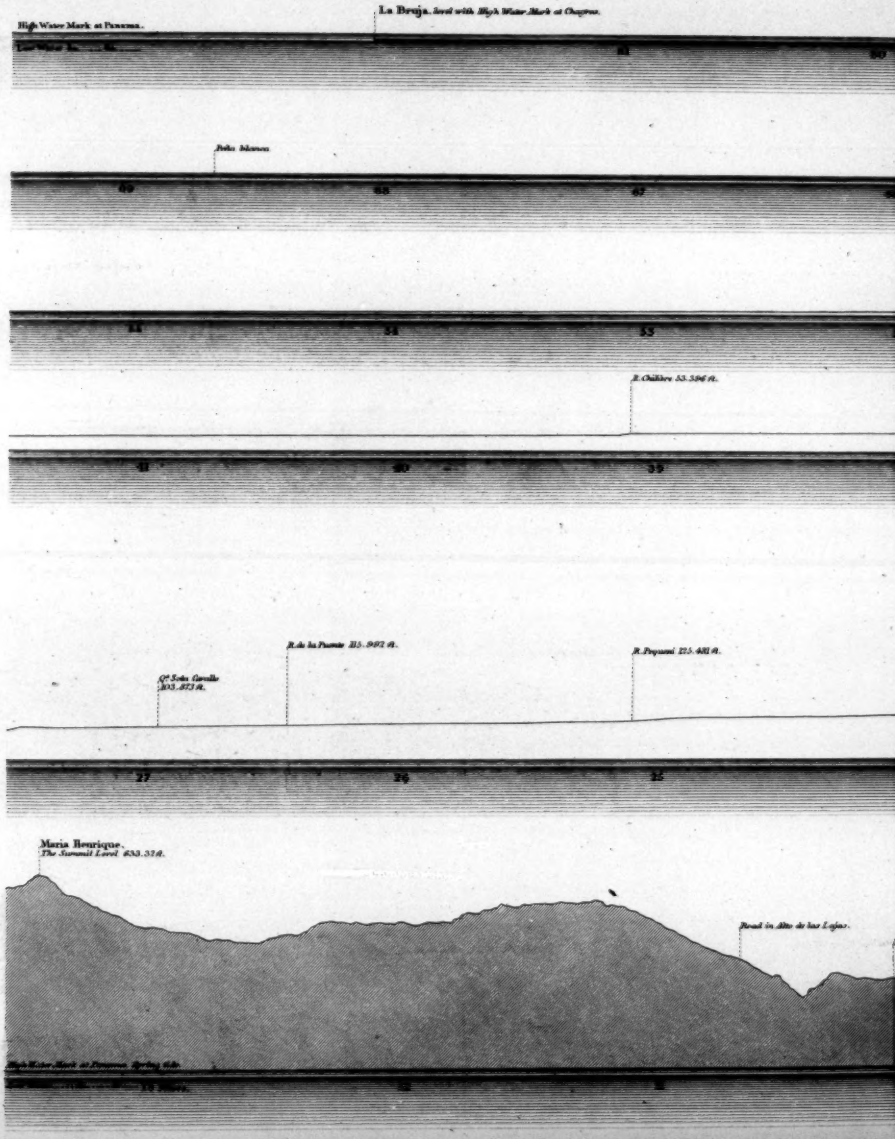
In a country in many respects so unsettled, it will readily be imagined that the authority and countenance, derived from a Government far from the spot, are not alone sufficient to enable a foreigner to carry through operations so extensive and long-continued as mine were. I am sensible that I could not have completed them, had it not been for the frequent assistance and constant support, which I received from the friendship of Malcolm MacGregor, Esq. H. M. consul at Panamá.

PLAN
of that Part of the
ISTHMUS of PANAMA,
Eligible for effecting a Communication
BETWEEN
The Atlantic & Pacific,
from Observations & Surveys performed in the Years 1828 & 1829.

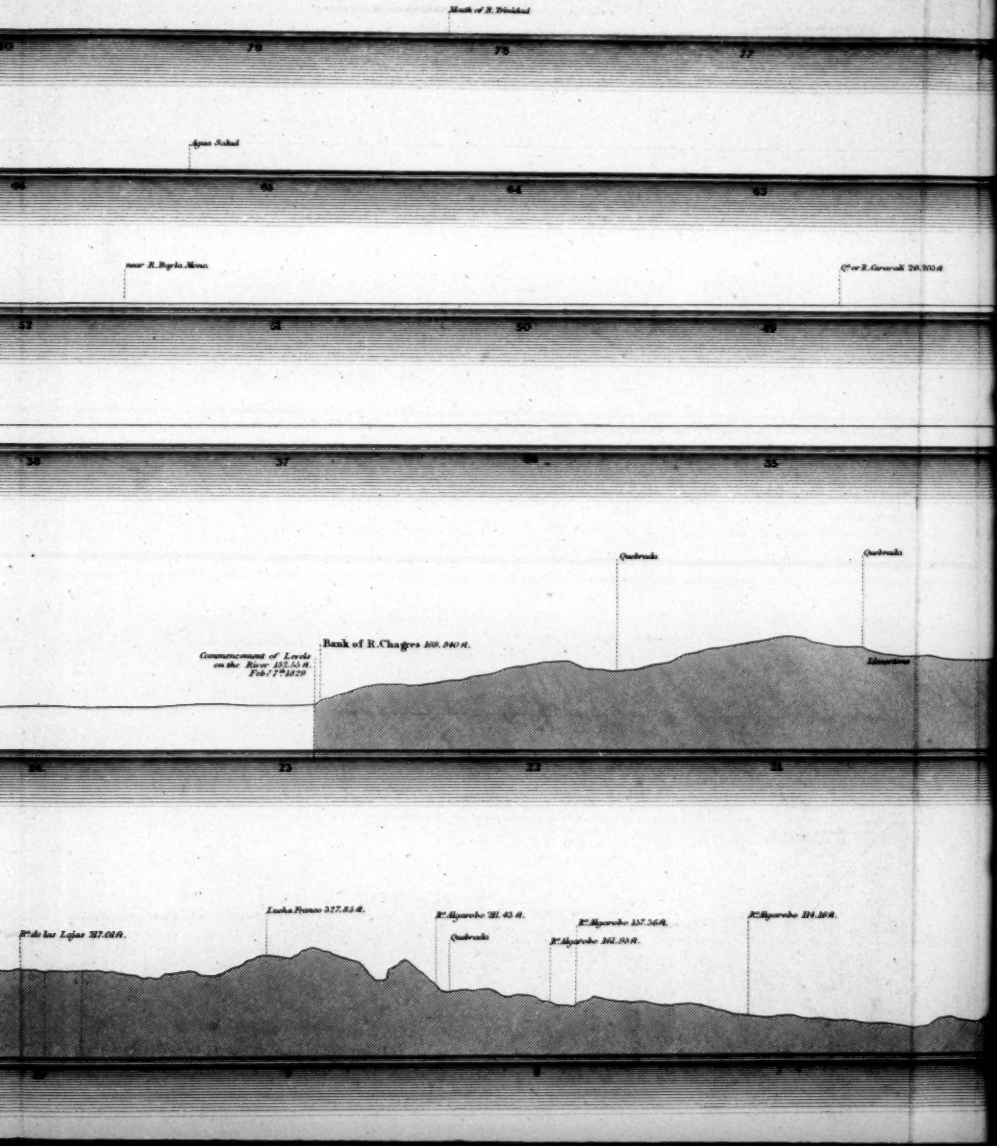
By J. A. LLOYD.



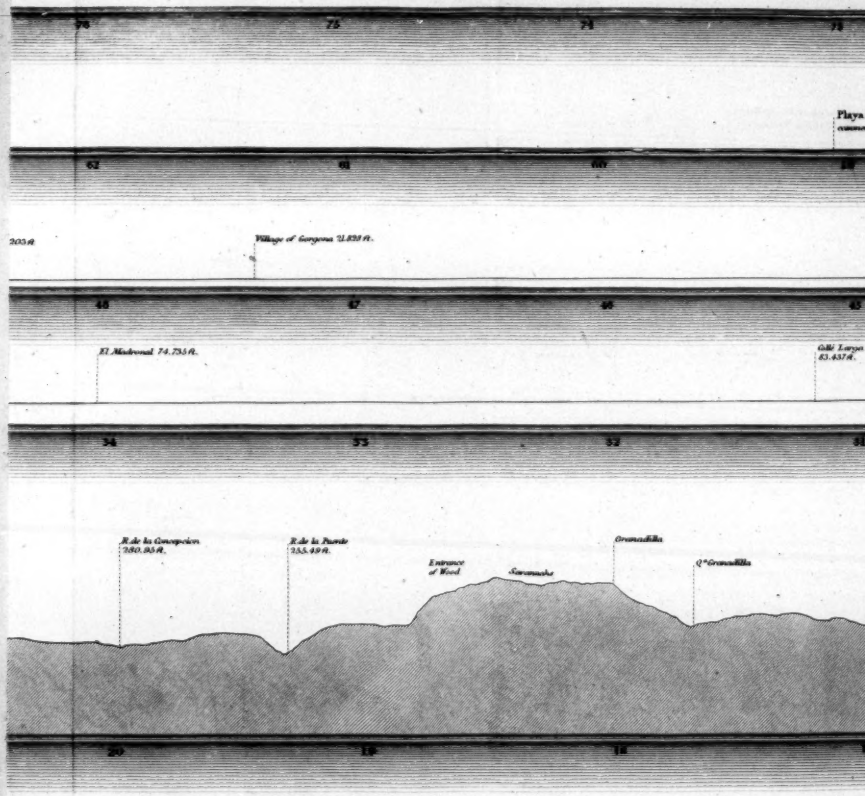
PANAMA
Bay of Chorrera
Isl. Naos
Isl. Perico
Isl. Flamenco
Isl. Venado
Isl. Gorgona
Bay of Panama
PACIFIC OCEAN

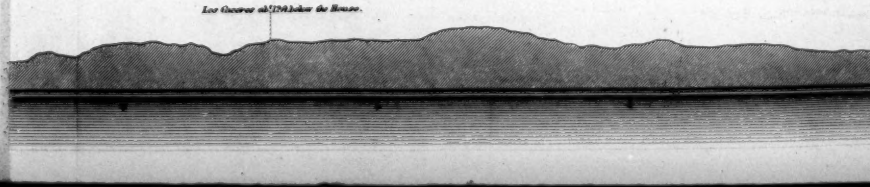

La Brujа. level with High Water Mark at Chagres.
High Water Mark at Panama.
Pedro Miguel
E. Culebra 53.596 ft.
R. de la Puente 115.997 ft.
R. Paquení 215.681 ft.
Cº Sobre Gavilla 303.873 ft.
Maria Enrique.
The Summit Level 653.51 ft.
Road in Alto de las Lajas.

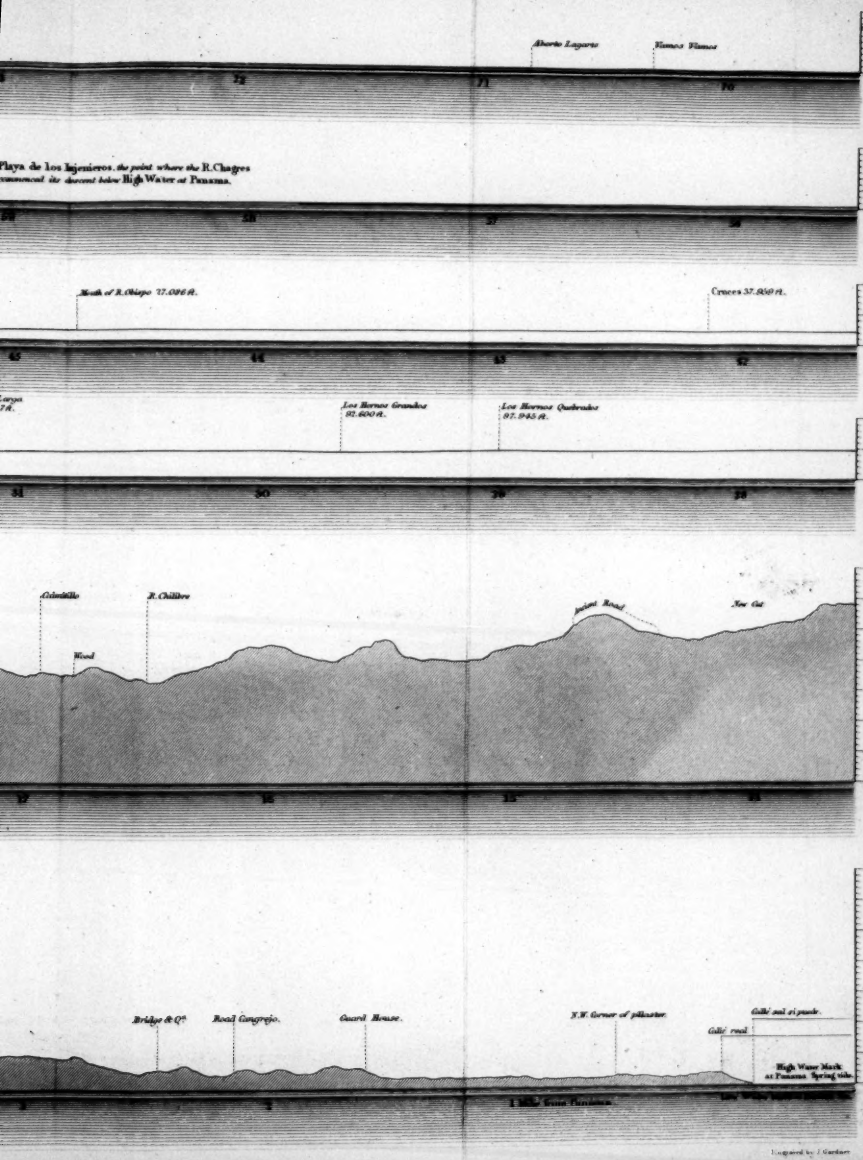
TION OF THE COUNTRY PASSED OVER, IN ASCERTAINING THE DIFFERENCE OF LEVEL



EVEL BETWEEN THE PACIFIC AND ATLANTIC OCEANS.







Engraved by J. Gardner

III. *On the law of the partial polarization of light by reflexion.* By DAVID BREWSTER, *LL.D. F.R.S. L. & E.*

Read February 4, 1830.

IN the year 1815 I communicated to the Royal Society a series of experiments on the polarization of light by successive reflexions, which contain the germ of the investigations, the results of which I now propose to explain.

From these experiments it appeared that a given pencil of light could be wholly polarized at any angle of incidence, provided it underwent a sufficient number of reflexions, either at angles wholly above or wholly below the maximum polarizing angle, or at angles partly above and partly below that angle; and it was scarcely possible to resist the conclusion, that the light not polarized by the first reflexion had suffered a physical change at each action of the reflecting force which brought it nearer and nearer to the state of complete polarization. This opinion, however, which I have always regarded as demonstrable, appeared in a different light to others. Guided probably by an experimental result, apparently though not really hostile to it, Dr. YOUNG and MM. BIOT, ARAGO, and FRESNEL, have adhered to the original opinion of MALUS, that the reflected and refracted pencils consist partly of light wholly polarized, and partly of light in its natural state; and more recently Mr. HERSCHEL has given the weight of his opinion to the same view of the subject.

Under these circumstances I have often returned to the investigation with renewed zeal; but though the frequent repetition of my experiments has more and more convinced me of the truth of the conclusions which I drew from them, yet I have not till lately been able to place the subject in a satisfactory aspect, and to connect it with general laws, which give a mathematical form to this fundamental branch of the science of polarization.

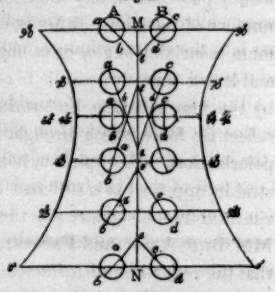
If we consider a pencil of natural light as divided into two pencils polarized in rectangular planes by the action of a doubly refracting crystal, and conceive the light of these two pencils to return back through the crystal, it will obviously emerge in the state of natural light. When we examine the pencil thus recomposed, or when we examine a pencil consisting of two oppositely polarized pencils superposed, we shall find that they comport themselves under every

analysis exactly like common light; so that we are entitled to assume such a pencil as the representative of natural light, and to consider every thing that can be established respecting the one, as true respecting the other.

In applying this principle to the analysis of the phænomena produced by reflexion, I placed the planes of polarization of the compound beam in the plane of reflexion; but though this led to some interesting conclusions, it did not develope any general law. I then conceived the idea of making the plane of reflexion bisect the right angle formed by the planes of polarization; and in this way I observed a series of symmetrical effects at different angles of incidence, which threw a broad light over the whole subject.

In order to explain these results, let AB (Fig. 1.) represent the two pencils of oppositely polarized light as separated by double refraction; let *a b*, *c d* be the directions of their planes of polarization, forming a right angle *a e c*, and let the plane of reflexion MN, of a surface of plate glass, bisect the angle *a e c*, so that the planes *a b*, *c d* form angles of + 45° and − 45° with the plane M N. Let a rhomb of calcareous spar have its principal section now placed in the plane of reflexion.

Fig. 1.



At an incidence of 90°, reckoned from the perpendicular, the reflected images of A and B suffer no change, the angle *a e c* is still a right angle, and the four pencils formed by the calcareous spar are all of equal intensity. As the incidence however diminishes, the angle *a e c* diminishes also, and the ordinary and extraordinary images of A and B differ in intensity. At an incidence of 80° for example, the angle *a e c* is reduced from 90° to 66°; at 70° it has been reduced to 40°, and at 56° 45′, the maximum polarizing angle, it has been reduced to 0°; that is, the planes of polarization *a b*, *c d* are now parallel. Below the polarizing angle, at 50°, the axes are again inclined to each other, and form an angle of 22°. At 40° they form an angle of 50°, and at 0°, or a perpendicular incidence, they are again brought back to their primitive inclination of 90°. Taking M N to represent the quadrant of incidence from 90° at M, to 0° at N, the curves, 90°, 0°, show the progressive change which takes place in the planes of polari-

zation, the plane of polarization being a tangent to the curve at the incidence which corresponds to any particular point of it.

When we employ a surface of diamond in place of glass, the inclination of the axes $a\,b, c\,d$ is reduced to 46° at an incidence of 80°, to 8° at an incidence of 70°, and at 67° 43′ the axes become parallel.

Such being the action of the reflecting forces upon A and B taken separately, let us now consider them as superposed and forming natural light. At 90° and 0° of incidence, the reflecting force produces no change in the inclination of their axes or planes of polarization; but at 56° 45′ in the case of glass, and 67° 43′ in the case of diamond, the axes of all the particles are brought into a state of parallelism with the plane of reflexion; and consequently when the image which they form is viewed by the rhomb of calcareous spar, they will all pass into the ordinary image, and thus prove that they are wholly polarized in the plane of reflexion.

All this is entirely conformable to what has been long known: but we now see that the total polarization of the reflected pencil at an angle whose tangent is the index of refraction, is effected by turning round the planes of polarization of one half of the light from right to left, and of the other half from left to right, each through an angle of 45°. Let us now see what takes place at those angles where the pencil is only partially polarized. At 80° for example, the angle of the planes $a\,b, c\,d$ is 66°, that is, each plane of polarization has been turned round in opposite directions from an inclination of 45° to one of 33° with the plane of reflexion. The light has therefore suffered a physical change of a very marked kind, constituting now neither natural nor polarized light. It is not natural light, because its planes of polarization are not rectangular; it is not polarized light, because they are not parallel. It is a pencil of light having the physical character of one half of its rays being polarized at an angle of 66° to the other half. It will now be asked, how a pencil thus characterized can exhibit the properties of a partially polarized pencil, that is, of a pencil part of whose light is polarized in the plane of reflexion, while the rest retains its condition of natural light. This will be understood by replacing the analysing rhomb with its principal section in the plane of reflexion, and viewing through it the images A and B at 80° of incidence. As the axis of A is inclined 33° to MN or the section of the rhomb, the ordinary image of it will be much brighter

than the extraordinary image, the intensity of each being in the ratio of $\cos^2 \varphi$ to $\sin^2 \varphi$, φ being the angle of inclination, or 33° in the present case. In like manner the ordinary image of B will be in the same ratio brighter than its extraordinary image, that is, by considering A and B in a state of superposition, the extraordinary image of a pencil of light reflected at 80° will be fainter than the ordinary image in the ratio of $\sin^2 33°$ to $\cos^2 33°$. But this inequality in the intensity of the two pencils is precisely what would be produced by a compound pencil, part of which is polarized in the plane of reflexion, and part of which is common light. When MALUS, therefore, and his successors analysed the pencil reflected at 80°, they could not do otherwise than conclude that it was partially polarized, consisting partly of light polarized in the plane of reflexion, and partly of natural light. The action of successive reflexions, however, afforded a more precise means of analysis, in so far as it proved that the portion of what was deemed natural light had in reality suffered a physical change, which approximated it to the state of polarized light; and we now see that the portion of what was called polarized light was only what may be called apparently polarized; for though it disappears, like polarized light, from the extraordinary image of the analysing prism, yet there is not a single particle of it polarized in the plane of reflexion.

These results must be admitted to possess considerable interest in themselves; but, as we shall proceed to show, they lead to conclusions of general importance. The quantity of light which disappears from the extraordinary image, is obviously the quantity of light which is really or apparently polarized at the given angle of incidence; and if we admit the truth of the law of repartition discovered by MALUS, and represented by $P_{oo} = P_o \cos^2 \varphi$, and $P_{oe} = P_o \sin^2 \varphi$, and if we can determine φ for substances of every refractive power, and for all angles of incidence, we may consider as established the mathematical law which determines the intensity of the polarized pencil, whatever be the nature of the body which reflects it,—whatever be the angle at which it is incident,—whatever be the number of reflexions which it suffers, and whether these reflexions are all made from one substance, or partly from one substance and partly from another.

The first step in this investigation is to determine the law according to which a reflecting surface changes the plane of polarization of a polarized ray. This

subject was first examined by MALUS, but not with that success which attended most of his labours. Before I was acquainted with what had been done by M. FRESNEL, or with the experiments of M. ARAGO on glass and water, I had made a number of very careful experiments on the same subject, and had represented them by formulæ founded on the law of the tangents. These formulæ, however, I found to be defective; and I am persuaded, from a very extensive series of experiments, that the formulæ of FRESNEL are accurate expressions of the phenomena under every variation of incidence and refractive power. If i is the angle of incidence, i' the angle of refraction, x the primitive inclination of the plane of the polarized ray to the plane of reflexion, and φ the inclination to which that plane is brought by reflexion, then, according to FRESNEL, we have

$$\text{Tan } \varphi = \tan x \frac{\cos (i + i')}{\cos (i - i')}$$

When x is 45°, as in the preceding observations, then tan $x = 1$, and we have

$$\text{Tan } \varphi = \frac{\cos (i + i')}{\cos (i - i')}.$$

In these formulæ, which are founded on the law of the tangents, $i + i'$ is the supplement of the angle which the reflected ray forms with the refracted ray; while $i - i'$ is the angle which the incident ray forms with the refracted ray, or the deviation produced by refraction.

These formulæ have been verified by M. ARAGO at ten angles of incidence upon Glass, and four upon Water; but his experiments were made only in the case where x is 45°, and where tan x disappears from the formula. As my experiments embrace a wider range of substances, and also the general case where x varies from 0° to 90°, I consider them as a necessary basis for a law of such extensive application.

The first series of experiments which I made was upon Plate Glass, in which the maximum polarizing angle was nearly 56°: hence I assume the index of refraction to be 1.4826. The following were the results:

Plate Glass.

Angle of Incidence.	Angle of Refraction.	Inclination of Plane of Polarization to Plane of Reflexion. Observed.	Computed.	Difference.
90°	0° 0′	45° 0′	45° 0′	0° 0′
88	42 23	43 4	42 49	+0 35
86	42 17	40 43	40 36	+0 7
84	42 8	38 47	38 22	+0 25
80	41 37	33 13	33 46	−0 33
75	40 40	28 45	27 41	+1 4
70	39 20	22 6	21 3	+1 3
65	37 41	14 40	13 53	+0 47
60	35 45	6 10	6 16	−0 6
56	34 0	0 0	0 0	0 0
50	31 22	9 0	9 0	0 0
45	28 29	16 55	16 31	+0 24
40	25 42	22 37	23 1	−0 24
30	19 43	32 25	33 19	−0 54
20	13 20	39 0	40 4	−1 4
10	6 44	44 0	43 49	+0 11

These results, obtained in every part of the quadrant, completely establish the accuracy of the formula. The differences are all within the limits of the errors of observation, and amount, at an average, to $32\frac{1}{2}'$ on each observation.

It is a curious circumstance, which I believe has not before been remarked, that at an incidence of 45° the deviation produced by refraction, or $i-i'$, is, in every substance, the complement of the angle of refraction i to 45°; and in the action of all substances upon polarized light at an incidence of 45°, the rotation of the plane of polarization of a pencil polarized + 45°, or − 45°, is equal to the angle of refraction; while the inclination of the plane of polarization to the plane of reflexion, or φ, is equal to the deviation $i-i$.

In order to establish the accuracy of the formula for different degrees of refractive power, I made the following experiments on Diamond, in which the index of refraction was 2.440.

DIAMOND.

Angle of Incidence.	Angle of Refraction.	Inclination of Plane of Polarization to Plane of Reflexion. Observed.	Calculated.	Difference.
90° 0′	24° 12′	45° 0′	45° 0′	0° 0′
85 0	24 6	34 30	33 56	+0 34
80 0	23 48	24 0	23 12	+0 48
75 0	23 19	14 30	13 8	+1 22
70 0	22 39	4 30	3 54	+0 36
67 43	22 17	0 0	0 0	0 0
60 0	20 47	12 30	11 41	+0 49
50 0	18 18	24 0	23 30	+0 30

These differences, which at an average amount to $46\frac{1}{2}'$, are also within the limits of the errors of observation.

In all these experiments the value of x was 45°; but in order to determine the law of variation for φ, when x varies from 0° to 90°, I took a crystal of quartz with a fine natural surface parallel to its axis; and I found that at an angle of incidence of 75°, and when x was 45°, the inclination of the plane of polarization to the plane of reflexion was 26° 20′. I then varied x, and obtained the following results:

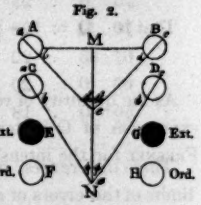
Values of x.	Inclination of Plane of Polarization. φ Observed.	φ Calculated.	Difference.
0°	0° 0′	0° 0′	0° 0′
10	4 54	4 29	+0 25
20	10 0	10 16	−0 16
30	15 50	16 2	−0 12
35	20 0	19 12	+0 48
40	23 30	22 40	+0 50
45	26 20	26 27	−0 7
50	30 0	30 40	−0 40
55	35 30	35 23	+0 7
60	40 0	40 45	−0 45
70	53 0	53 49	−0 49
80	70 0	70 29	−0 29
90	90 0	90 0	0 0

In these experiments the average error does not exceed half a degree. The third column is computed by the formula $\tan \varphi = (\tan 26° 27') \tan x$.

From these experiments it appears that the formula expresses with great accuracy all the changes in the planes of polarization which are produced by a single reflexion, and we may therefore apply it in our future investigations.

Let us now suppose that a beam of common light composed of two portions A, B, (Fig. 2.) polarized + 45° and − 45° to the plane of reflexion, is incident on a plate of glass at such an angle that the reflected pencil composed of C and D has its planes of polarization inclined at an angle φ to the plane M N. When a rhomb of calcareous spar has its principal section in the plane M N, it will divide the image C into an extraordinary pencil E and an ordinary one F; and the same will take place with D, G being its extraordinary and H its ordinary image. If we represent the whole of the reflected pencil or C + D by 1, then $C = \frac{1}{2}$, $D = \frac{1}{2}$, E + F = 1, and G + H = 1. But since the planes of polarization of C and D are each inclined φ degrees to the principal section of the rhomb, the intensity of the light of the doubly refracted pencils will be as $\sin^2 \varphi : \cos^2 \varphi$; that is, the intensity of E will be $\frac{1}{2} \sin^2 \varphi$, and that of F, $\frac{1}{2} \cos^2 \varphi$. Hence it follows that the difference of these pencils, or $\frac{1}{2} \sin^2 \varphi - \frac{1}{2} \cos^2 \varphi$, will express the quantity of light which has passed from the extraordinary image E into the ordinary one F, that is, the quantity of light apparently polarized in the plane of reflexion M N. But as the same is true of the pencil D, we have $2 (\frac{1}{2} \sin^2 \varphi - \frac{1}{2} \cos^2 \varphi)$ or $\sin^2 \varphi - \cos^2 \varphi$ for the whole of the polarized light in a pencil of common light C + D. Hence, since $\sin^2 \varphi + \cos^2 \varphi = 1$ and $\cos^2 \varphi = 1 - \sin^2 \varphi$, we have for the whole quantity of polarized light

Fig. 2.



$$Q = 1 - 2 \sin^2 \varphi.$$

But $$\operatorname{Tan} \varphi = \tan x \frac{\cos (i + i')}{\cos (i - i')}$$

And as $$\operatorname{Tan}^2 \varphi = \frac{\sin^2 \varphi}{\cos^2 \varphi}, \text{ and } \sin^2 \varphi + \cos^2 \varphi = 1,$$

we have the quotient and the sum of the quantities $\sin^2\varphi$ and $\cos^2\varphi$, by which we obtain

$$\mathrm{Sin}^2\varphi = \frac{1}{\left(\tan x \frac{\cos(i+i')}{\cos(i-i')}\right)^2 + 1} = \frac{\left(\tan x \frac{\cos(i+i')}{\cos(i-i')}\right)^2}{1+\left(\tan x \frac{\cos(i+i')}{\cos(i-i')}\right)^2}$$

That is $$\mathrm{Q} = 1 - 2\frac{\left(\tan x \frac{\cos(i+i')}{\cos(i-i')}\right)^2}{1+\left(\tan x \frac{\cos(i+i')}{\cos(i-i')}\right)^2}$$

As the quantity of reflected light is here supposed to be 1, we may obtain an expression of Q in terms of the incident light by adopting the formula of FRESNEL for the intensity of a reflected ray. Thus

$$\mathrm{Q} = \tfrac{1}{2}\left(\frac{\sin^2(i-i')}{\sin^2(i+i')} + \frac{\tan^2(i-i')}{\tan^2(i+i')}\right)\left(1 - 2\frac{\left(\frac{\cos(i+i')}{\cos(i-i')}\right)^2}{1+\left(\frac{\cos(i+i')}{\cos(i-i')}\right)^2}\right)$$

As $\tan x = 1$ in common light, it is omitted in the preceding formula.

This formula may be adapted to partially polarized rays, that is, to light reflected at any angle different from the angle of maximum polarization, provided we can obtain an expression for the quantity of reflected light.

M. FRESNEL's general formula has been adapted to this species of rays, by considering them as consisting of a quantity a of light completely polarized in a plane making the angle x with that of incidence, and of another quantity $1 - a$ in the state of natural light. Upon this principle it becomes

$$\mathrm{I} = \frac{\sin^2(i-i')}{\sin^2(i+i')} \cdot \frac{1+a\cos^2 x}{2} + \frac{\tan^2(i-i')}{\tan^2(i-i')} \cdot \frac{1-a\cos^2 x}{2}.$$

But as we have proved that partially polarized rays are rays whose planes of polarization form an angle of $2x$ with one another as already explained, x being greater or less than 45°, we obtain a simpler expression for the intensity of the reflected pencil, viz. the very same as that for polarized light.

$$\mathrm{I} = \frac{\sin^2(i-i')}{\sin^2(i+i')}\cos^2 x + \frac{\tan^2(i-i')}{\tan^2(i+i')}\sin^2 x$$

Hence we have

$$Q = \left(\frac{\sin^2(i-i')}{\sin^2(i+i')}\cos^2 x + \frac{\tan^2(i-i')}{\tan^2(i+i')}\sin^2 x\right)\left(1 - 2\frac{\left(\tan x \frac{\cos(i+i')}{\cos(i-i')}\right)^2}{1+\left(\tan x \frac{\cos(i+i')}{\cos(i-i')}\right)^2}\right)$$

This formula is equally applicable to a single pencil of polarized light of the same intensity as the pencil of partially polarized light. In all these cases it expresses the quantity of light really or apparently polarized in the plane of reflexion.

In order to show the quantity of light polarized at different angles of incidence, I have computed the following table for common light, and suited to glass in which $m = 1.525$.

PLATE GLASS.

Angle of Incidence i.	Angle of Refraction i'.	Inclination of Plane of Polarization to Plane of Reflexion, ϕ.	Quantity of Light reflected out of 1000 Rays.	Quantity of Polarized Light Q.	Ratio of Polarized to Reflected Light.
° ′	° ′	° ′			
0 0	0 0	45 0	43.23	0	0
10 0	6 32	43 51	43.39	1.74	0.04000
20 0	12 58	40 13	43.41	7.22	0.16618
25 0	16 5	37 21	43.64	11.6	0.26388
30 0	19 8½	33 40	44.78	17.25	0.3853
35 0	22 6	29 8	46.33	24.37	0.5260
40 0	24 56	23 41	49.10	33.25	0.6773
45 0	27 37½	17 22½	53.66	44.09	0.82167
50 0	30 9	10 18	61.36	57.36	0.9360
56 45	33 15	0 0	79.5	79.5	1.000
60 0	34 36	5 4½	93.31	91.6	0.9628
65 0	36 28	12 45	124.86	112.7	0.90258
70 0	38 2	18 32	162.67	129.80	0.79794
75 0	39 18	26 52	257.26	152.34	0.59154
78 0	39 54	30 44	329.95	157.67	0.47786
79 0	40 4	31 59	359.27	157.69	0.43892
80 0	40 13	33 13	391.7	156.6	0.40000
82 44	40 35	36 22	499.44	145.4	0.29112
84 0	40 42	38 2	560.32	134.93	0.2408
85 0	40 47	39 12	616.28	123.75	0.2008
86 0	40 51	40 22.7	676.26	108.67	0.16068
87 0	40 54	41 32	744.11	89.83	0.12072
88 0	40 57½	42 42	819.9	65.9	0.0804
89 0	40 58	43 51	904.81	36.32	0.04014
90 0	40 58	45 0	1000.0	0	0.0000

As the preceding formula is deduced from principles which have been either established by experiment or confirmed by it, it may be expected to harmonize

with the results of observation. At all the limits where the pencil is either wholly polarized or not polarized at all, it of course corresponds with experiment: but though in so far as I know there have been no absolute measures taken of the quantity of polarized light at different incidences, yet we are fortunately in possession of a set of experiments by M. Arago, who has ascertained the angles above and below the polarizing angle at which glass and water polarize the same proportion of light. In no case has he measured the absolute quantity of the polarized rays; but the comparison of the values of Q at those angles at which he found them in equal proportions, will afford a test of the accuracy of the formula. This comparison is shown in the following table, in which col. 1. contains the angles at which the reflecting surface polarizes equal proportions of light; col. 2. the values of φ or the inclination of the planes of polarization; and col. 3. the intensities of the polarized light computed from the formula.

	Angles of Incidence i.	Inclination of Planes of Polarization to M N, or φ.	Proportion of Polarized Light or Q.
Glass: No. 1.	82° 48′	37° 33′	.2572
	24 18	37 21	.2637
No. 2.	82 5	36 47	.2828
	26 6	36 0	.3090
No. 3.	78 20	32 38	.4186
	29 42	33 1	.4064
Water: No. 4.	86 31	41 54	.1080
	16 12	41 27	.1236

The agreement of the formula with experiments made with as great accuracy as the subject will admit must be allowed to be very satisfactory. The differences are within the limits of the errors of observation, as appears from the following table:

	Deviations from Experiment.	Part of the whole Light.
Glass: No. 1.	0.0065	$\frac{1}{154}$
No. 2.	0.0262	$\frac{1}{38}$
No. 3.	0.0122	$\frac{1}{82}$
Water: No. 4.	0.0156	$\frac{1}{64}$

M. Arago has concluded, from the experiments above stated, that equal proportions of light are polarized at equal angular distances from the angle of

complete polarization. Thus in Glass No. 1. the mean of 82° 48′ and 24° 18′ is 53° 33′, which does not differ widely from the maximum polarizing angle, or 55°, which M. Arago considers as the maximum polarizing angle of the glass *. In order to compare this principle with the formula, I found that in Water No. 4. the angle which polarizes almost exactly the same proportion of light as the angle of 86° 31′, is 15° 10′, the value of φ being 41° 54′ at both these angles; but the mean of these is 50° 50′ in place of 53° 11′; so that the rule of M. Arago cannot be regarded as correct, and cannot therefore be employed, as he proposes, to determine the angle of complete polarization †.

The application of the law of intensity to the phenomena of the polarization of light by successive reflexions, forms a most interesting subject of research. No person, so far as I know, has made a single experiment upon this point, and those which I have recorded in the Philosophical Transactions for 1815, have, I believe, never been repeated. All my fellow labourers, indeed, have overlooked them as insignificant, and have even pronounced the results which flow from them to be chimerical and unfounded. Those immutable truths, however, which rest on experiment, must ultimately have their triumph; and it is with no slight satisfaction, that, after fifteen years of unremitted labour, I am enabled not only to demonstrate the correctness of my former experiments, but to present them as the necessary and calculable results of a general law.

When a pencil of common light has been reflected from a transparent surface, at an angle of 61° 3′ for example, it has experienced such a physical change, that its planes of polarization form an angle of 6° 45′ each with the plane of reflexion. When it is incident on another similar surface at the same angle, it is no longer common light in which $x = 45°$, but it is partially polarized light in which $x = 6° 45'$. In computing therefore the effect of the second reflexion, we must take the general formula $\tan \varphi = \tan x \left(\frac{\cos (i + i')}{\cos (i - i')}\right)$; but, as the value of x is always in the same ratio to the value of φ, however great be the number of reflexions, we have $\tan \theta = \tan^n \varphi$ for the inclination θ to the plane of reflexion produced by any number of reflexions n,

* Hence we have assumed $m = 1.428$, the tangent of 55°, in the preceding calculations.

† It is obvious that the rule can only be true when $m = 1.000$; so that its error increases with the refractive power.

φ being the inclination for one reflexion. Hence when θ is given by observation, we have $\tan \varphi = \sqrt[n]{\theta}$. The formula for any number n of reflexions is therefore $\tan \theta = \left(\frac{\cos (i + i')}{\cos (i + i)}\right)^n$. It is evident that θ never can become equal to 0°; that is, that the pencil cannot be so completely polarized by any number of reflexions at angles different from the polarizing angle, as it is by a single reflexion at the polarizing angle; but we shall see that the polarization is sensibly complete in consequence of the near approximation of θ to 0°.

I found, for example, that light was polarized by two reflexions from glass at an angle of 61° 3′, and 60° 28′ by another observation. Now in these cases we have

	θ after 1st Reflexion.	θ after 2nd Reflexion.	Quantity of Unpolarized Light.
Two reflexions at 61° 3′	6° 45′	0° 47′	0.00037
60 28	5 38	0 33	0.00018

The quantity of unpolarized light is here so small as to be quite inappreciable with ordinary lights.

In like manner I found that light was completely polarized by five reflexions at 70°. Hence by the formula we have

	Values of θ.	Unpolarized Light.
1 reflexion at 70°	20° 0′	0.23392
2	7 32	0.03432
3	2 45	0.00460
4	1 0	0.00060
5	0 22	0.00008

The quantity of unpolarized light is here also unappreciable after the 5th reflexion.

In another experiment I found that light was wholly polarized by the separating surface of glass and water at the following angles:

	Values of θ.	Unpolarized Light.
By 2 reflexions at 44° 51′	0° 56′	0.0005
By 3 42 27	0 26	0.0001

In all these cases the successive reflexions were made at the same angle; but the formula is equally applicable to reflexions at different angles,—

1. When both the angles are greater than the polarizing angle.

	θ	Unpolarized Light.
1 reflexion at 58° 2′, and 1 at 67° 2′	0° 34′	0.0002

2. When one of the angles is above and the other below the polarizing angle.

		Unpolarized Light.
1 reflexion at 53°, and 1 at 58° 2′ . .	0° 12′ . .	0.000024

This experiment requires a very intense light, for I find in my journal that the light of a candle is polarized at 53° and 78°.

In reflexions at different angles, the formula becomes $\tan \theta = \frac{\cos (i + i')}{\cos (i - i')} \times \frac{\cos (I + I')}{\cos (I + I')}$, I and i being the angles of incidence. In like manner if a, b, c, d, e, &c. are the values of φ or θ for each reflexion, or rather for each angle of incidence, we shall have the final angle or $\tan \theta = \tan a \times \tan b \times \tan c \times \tan d$, &c.

It is scarcely necessary to inform the reader that when a pencil of light reflected at 58° 2′ is said to be polarized by another reflexion at 67° 2′, it only means, that this is the angle at which complete polarization takes place in diminishing the angle gradually from 90° to 67° 2′, and that even this angle of 67° 2′ will vary with the intensity of the original pencil, with the opening of the pupil, and with the sensibility of the retina. But when it shall be determined experimentally at what value of φ, or rather at what value of Q, the light entirely disappears from the extraordinary image, we shall be able by inverting the formula to ascertain the exact number of reflexions by which a given pencil of light shall be wholly polarized.

As the value of Q depends on the relation of i and i', that is, on the index of refraction, and as this index varies for the different colours of the spectrum, it is obvious that Q will have different values for these different colours. The consequence of this must be, that in bodies of high dispersive powers, the unpolarized light which remains in the extraordinary image, and also the light which forms the ordinary image, must be coloured at all incidences; the colours being most distinct near the maximum polarizing angle. This necessary result of the formula, I found to be experimentally true in oil of cassia, and various highly dispersive bodies. In realgar for example φ is $= 0$ at an angle of 69° 0′ for blue light, at 68° 37′ for green light, and at 66° 49′ for red light. Hence there can be no angle of complete polarization for white light, which I also found to be the case by experiment; and as Q must at different angles of incidence have different values for the different rays, the unpolarized light must be composed of a certain portion of each different colour, which may be easily determined by the formula.

Such are the laws which regulate the polarization of light by reflexion from the first surfaces of bodies that are not metallic. The very same laws are applicable to their second surfaces, provided that the incident light has not suffered previous or subsequent refraction from the first surface. The sine of the angle at which φ or Q has a certain value by reflexion from the second surface, is to the sine of the angle at which they have the same value at the first surface, as unity is to the index of refraction. Hence φ and Q may be determined by the preceding formulæ after any number of reflexions, even if some of the reflexions are made from the first surface of one body and the second surface of another.

When the second surface is that of a plate with parallel or inclined faces, its action upon light presents curious phenomena, the law of which I have determined. I refer of course to the action of the second surface at angles less than that which produces total reflexion. This action has hitherto remained uninvestigated. It has been hastily inferred, however, from imperfect data; and the erroneous inference forms the basis of some optical laws, which are considered to be fully established.

Among the various results of the preceding investigation, there is one which seems to possess some theoretical importance. If we consider polarized rays as those whose planes of polarization are parallel, then it follows that light cannot be brought into such a state by any number of reflexions, or at any angle of incidence, excepting at the angle of complete polarization. At all other angles the light which seems to be polarized, by disappearing from the extraordinary image of the analysing rhomb, is distinguished from really polarized light, by the property of its planes of polarization forming an angle with each other and with the plane of reflexion. At the polarizing angle, for example, of 56° 45′ in glass, the light reflected is 79.5 rays, and it is completely polarized, because the planes of polarization of all the rays are parallel; but at an angle of incidence of 80°, where 392 rays are reflected, no fewer than 157 appear to be polarized, though their planes of polarization are inclined 66° 26′ to each other, or 33° 13′ to the plane of reflexion. This appearance of polarization, when the rays have only suffered a displacement in their planes of polarization from an angle of 90°, which approximates them to the state of polarized light, arises from the law which regulates the repartition of polarized light between the ordinary and extraordinary images produced by double re-

fraction, and shows that the analysing crystal is not sufficient to distinguish light completely polarized from light in a state of approach to polarization. The difference, however, between these two kinds of light is marked by most distinctive characters, and will be found to show itself in some of the more complex phenomena of interference.

In my paper of 1815, already referred to, I was led by a distant view of the phenomena which I have now developed, to consider common light as composed of rays in every state of positive and negative polarization*; and upon this principle the whole of the phenomena described in this paper may be calculated with the same exactness as upon the supposition of two oppositely polarized pencils. Nothing indeed can be simpler than such a principle. The particles of light have planes, which are acted upon by the attractive and repulsive forces residing in solid bodies; and as these planes must have every possible inclination to a plane passing through the direction of their motion, one half of them will be inclined — to this plane, and the other half +. When light in such a state falls upon a reflecting surface, the — and the + particles have each their planes of polarization brought more or less into a state of parallelism with the plane of reflexion, in consequence of the action of the repulsive force upon one side or pole of the particle through which the plane passes; while in the particles which suffer refraction, the same sides or poles are by the action of the attractive force drawn downwards, so as to increase the inclination of their planes relative to the plane of incidence, and bring them more or less into a state of parallelism with a plane perpendicular to that of refraction.

The formulæ already given, and those for refracted light which are contained in another paper, represent the laws according to which the repulsive and attractive forces change the position of the planes of polarization; and as we have proved that the polarization is the necessary consequence of these planes being brought into certain positions, we may regard all the various phenomena of the polarization of light by reflexion and refraction, as brought under the dominion of laws as well determined as those which regulate the motions of the planets.

Allerly, December 25, 1829.

* M. Biot has followed me in this opinion. See *Traité de Physique*, tom. iv. p. 304.

IV. *A Report on the stomach of the Zariffa.* By Sir Everard Home, *Bart., Vice-President of the Royal Society.*

Read December 24, 1829.

HAVING submitted to the King the following report upon the stomach of the Zariffa, I am sanctioned by His Majesty's entire approbation in laying it before the Royal Society.

The stomach consists of four cavities, like those of all quadrupeds that chew the cud. The internal surface of the paunch differs in nothing from that of the bullock, but in the projecting parts being more prominent. In the second cavity the cells met with in other ruminants are too superficial to retain water. The third and fourth cavities in every respect resemble those of the bullock. The more minute structures of these parts are distinctly shown in the annexed Plates.

As the only peculiarities in the Zariffa's stomach are in the second cavity, comparative views are given of this cavity with that in the bullock and sheep.

In considering the structure of the different parts of the stomach of ruminating animals, there can be no doubt that any peculiarity met with in particular tribes is to serve the purpose of enabling them more readily to subsist upon the food which is intended by nature for their use. Of this the reservoirs for water in the stomach of the camel are a remarkable instance. In the bullock and sheep the cud formed of grass in its return after the second mastication into the stomach is rendered dry, and when it drops into the second cavity requires being re-moistened before it can be spread between the membranous folds of the third cavity; this is effected by the water contained in the cells with which this cavity is furnished. In the Zariffa the cud formed from the leaves and twigs of the acacia is so succulent as not to require being again moistened in passing through the second stomach, and therefore that cavity in this animal is not furnished with cells of the same depth as in the other animals that chew the cud.

Explanation of the PLATES.

PLATE VI.

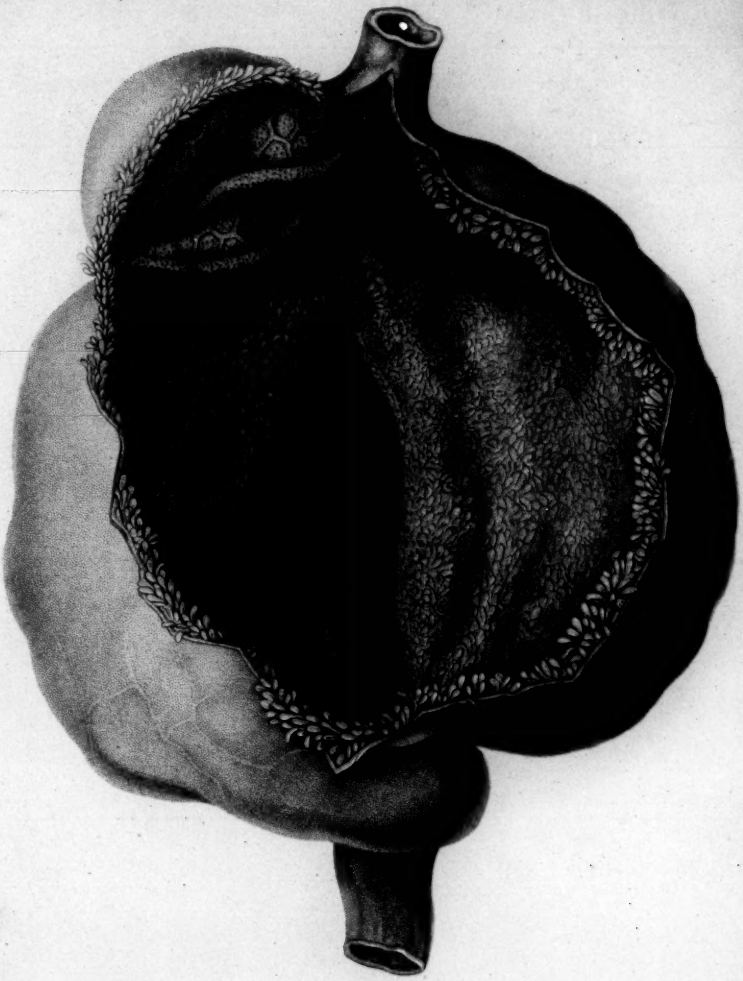
A front view of the stomach of the zariffa, on a reduced scale of 4 inches for a foot.

PLATE VII.

A back view of the same.

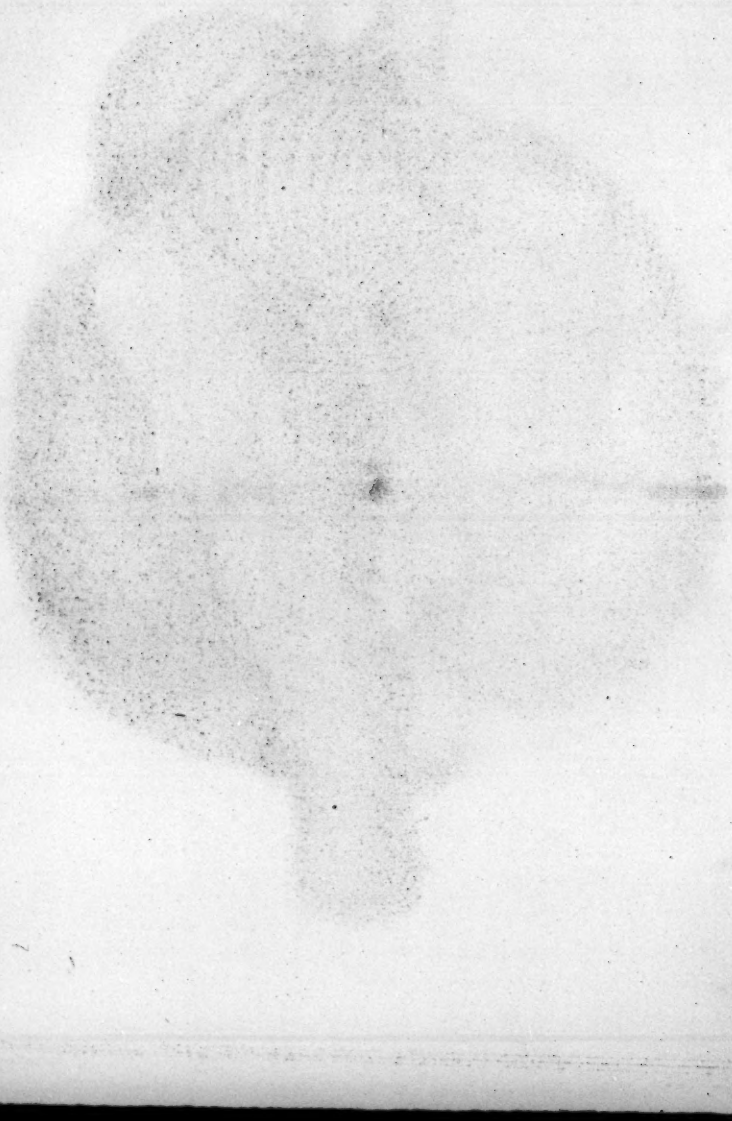
PLATE VIII.

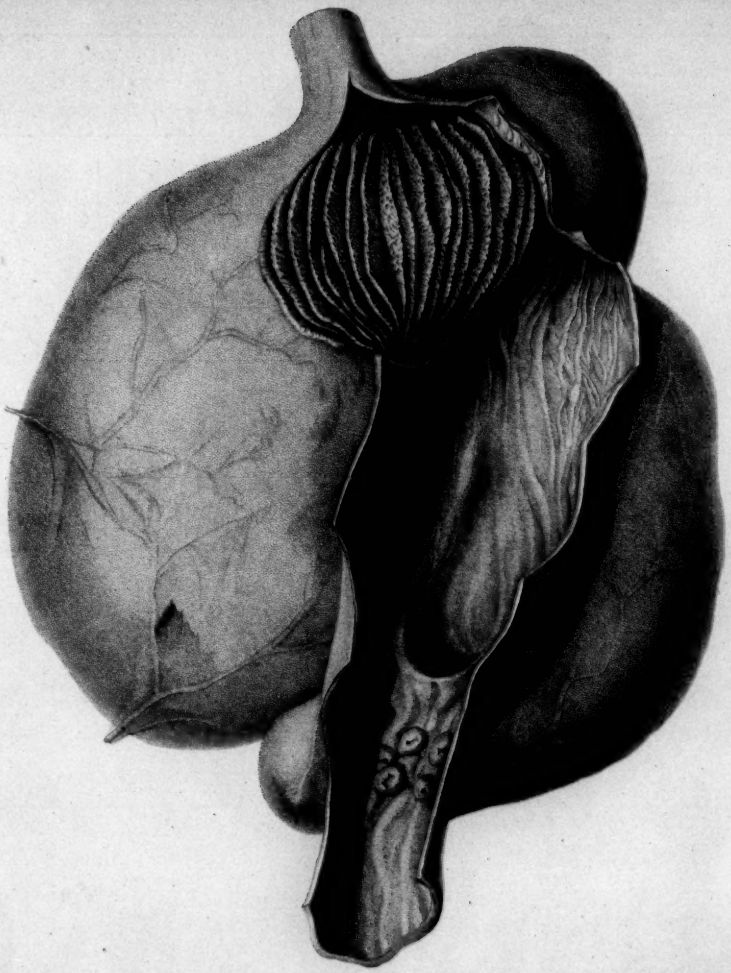
Fig. 1.—A portion of the first cavity of the zariffa's stomach; natural size.
Fig. 2.—A very small portion of the same; magnified 4 diameters.
Fig. 3.—A portion of the second cavity of the zariffa's stomach; natural size.
Fig. 4.—A very small portion of the same; magnified 4 diameters.
Fig. 5.—A portion of the third cavity of the zariffa's stomach; natural size.
Fig. 6.—A very small portion of the same; magnified 4 diameters.
Fig. 7.—A portion of the first cavity of the bullock's stomach; natural size.
Fig. 8.—A very small portion of the same; magnified 4 diameters.
Fig. 9.—A portion of the second cavity of the bullock's stomach; natural size.
Fig. 10.—A portion of the first cavity of the sheep's stomach; natural size.
Fig. 11.—A very small portion of the same; magnified 4 diameters.
Fig. 12.—A portion of the second cavity of the sheep's stomach, in which the cells are deeper than in the zariffa's stomach, but less so than in the bullock's stomach; natural size.



Drawn by Franc. Bauer 1819.

Engraved by James Basire.





Drawn by Frans. Bauer. 1820.

Engraved by James Basire.

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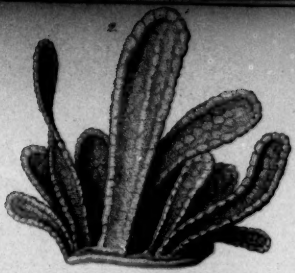
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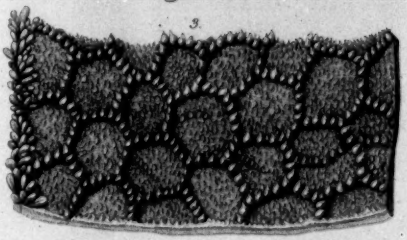
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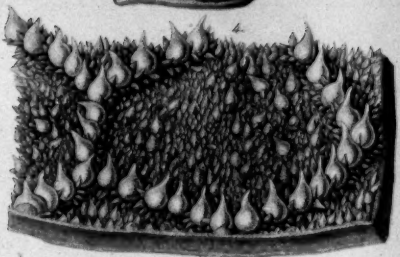
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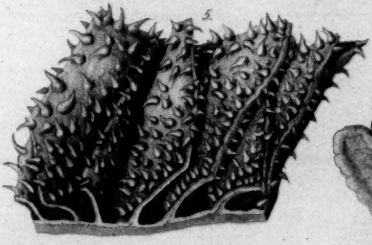
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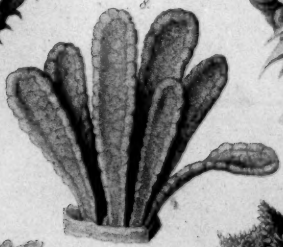
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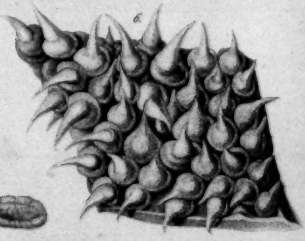
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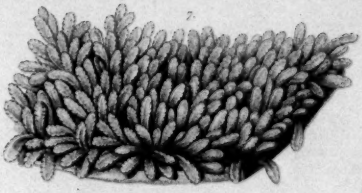
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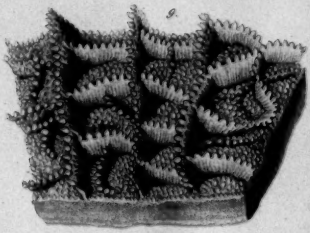
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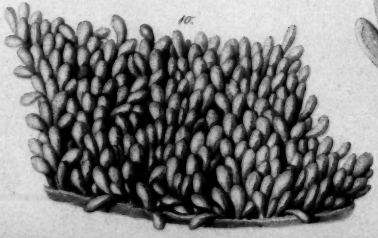
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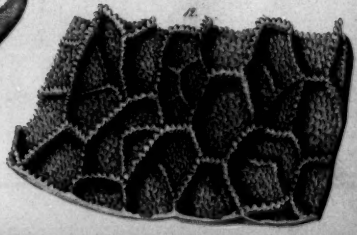
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Fig. 2.

Fig. 3.

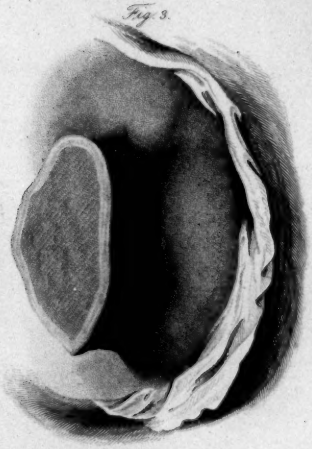
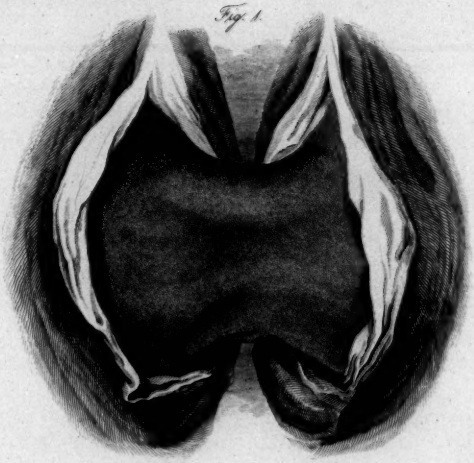


Fig. 1.



V. *On the production of regular double refraction in the molecules of bodies by simple pressure; with observations on the origin of the doubly refracting structure.* By DAVID BREWSTER, *LL.D. F.R.S. L. & E.*

Read February 11, 1830.

IN various papers already printed in the Philosophical Transactions, I have had occasion to show that the phenomena of double refraction may be produced artificially by certain changes in the mechanical condition of hard and soft solids*. In all these cases the phenomena are related to the form of the mass in which the change is induced; and in the case of hard and elastic solids, they vary with any variation of form which alters the mechanical state of the particles. In isinglass and other bodies to which double refraction has been communicated by induration, the particles take a permanent position, which is not altered by any change of shape; but still the phenomena exhibited by a given portion of the mass are related to the surfaces where the indurating cause operated, and also to those by which the isinglass was bounded; and they depend on the position which that portion occupies in the general mass.

In all these cases the phenomena are entirely different from those of regular crystals, and in none of them is the doubly refracting force a function of the angle which the incident ray forms with one or more axes given in position.

As long ago as 1814 I communicated to the Royal Society the following experiment on the depolarizing structure of white wax and resin:

"When resin is mixed with an equal part of white wax, and is pressed between two plates of glass by the heat of the hand, the film is almost perfectly transparent by transmitted light, though of a milky white appearance by reflected light. It has not the property of depolarization when the light is incident vertically; but it possesses it in a very perfect manner at an oblique incidence, and exhibits the segments of coloured rings†".

* Phil. Trans. 1814; 1815, pp. 1, 80, 60; 1816, pp. 46, 56.

† Ibid. 1815, pp. 31, 32.

The subject of double refraction was then so little developed that this experiment excited no notice; and it was only brought to my own recollection by the accidental appearance of the specimen itself. This depolarizing film has suffered no change by remaining fifteen years between the plates of glass. The vertical line along which it is destitute of the property of depolarization is a single axis of double refraction; and the coloured rings at oblique incidences are produced by the inclination of the refracted ray to the axis of double refraction. In order to examine this remarkable effect under a more general aspect, I made a considerable number of such plates with different kinds of wax, and with various proportions of resin, and I was led to results which seem to possess considerable interest.

When the white wax is melted alone and cooled between two plates of glass, it consists of a number of minute particles, each possessing double refraction, but having their axes turned in all possible directions. If the film of wax is made extremely thin, the particles are not sufficiently numerous to exhibit any action upon polarized light.

When resin alone is melted and cooled in a similar manner, it exhibits no doubly refracting structure, whether it indurates slowly or under the influence of pressure.

If resin and white wax are mixed in nearly equal proportions, the compound possesses considerable tenacity. When a proportion of it is melted and cooled between two plates of glass, it shows the quaquaversus polarization of bees'-wax, the axes of the elementary particles being turned in every direction. It possesses a considerable degree of opalescence, and a luminous body seen through it is surrounded with nebulous light. This imperfect transparency evidently arises from the reflexion and refraction of the rays in passing from one molecule to another, occasioned by a difference in the refractive power of the ingredients, or by the imperfect contact of the particles, or by both these causes combined.

In order to observe the modifications which these phenomena received from pressure, I took a few drops of the melted compound and placed them in succession on a plate of thick glass, so as to form a large drop. Before it was cold, I laid above the drop a circular piece of glass about two thirds of an inch in diameter, and by a strong vertical pressure on the centre of the piece of

glass, I squeezed out the drop into a thin plate. This plate was now almost perfectly transparent, as if the pressure had brought the particles of the substance into optical contact.

If we expose this plate to polarized light, we shall find that it possesses one axis of positive double refraction, and exhibits the polarized tints as perfectly as many crystals of the mineral kingdom. The structure thus communicated to the soft film by pressure does not belong to it as a whole, nor has it only one axis passing through its centre like a circular piece of unannealed glass. In every point of it there is an axis of double refraction perpendicular to the film, and the doubly refracting force varies with the inclination of the incident ray to this axis, as in all regular uniaxal crystals. When the two plates of glass are drawn asunder, we can remove one or more portions of the compressed film, and these portions act upon light exactly like films of uniaxal mica or hydrate of magnesia, and develope a doubly refracting force of equal intensity.

This remarkable experiment presents an interesting subject of inquiry. That the regular double refraction of the film is developed by the agency of pressure cannot be doubted; but it does not at first sight appear whether it is the immediate effect of the pressure, or is the same doubly refracting force which produces the quaquaversus polarization that takes place when the resinous film indurates without constraint. In this state of the film the axes of double refraction are clearly turned in every conceivable direction; and it is impossible to suppose that a pressure in one direction could suddenly arrange all these axes in parallel positions. The double refraction of each particle of the film has therefore been developed by the compressing force similarly applied to them; and in producing this effect, it must have deprived each particle of the doubly refracting structure which it previously possessed. The substitution of one doubly refracting structure for another may be easily effected in many bodies. Even in regular crystals we can by heat or pressure modify or remove their double refraction. Nay, we can take away one axis from a binaxal crystal, and communicate a second axis to an uniaxal one. When the doubly refracting structure is produced by induration, we can remove it wholly by pressure, and replace it with another even of an opposite character; and when it is generated by the living principle, as in the case of

the crystalline lenses of animals, we can take it away entirely, and substitute a new and more powerful doubly refracting structure by induration.

We may therefore consider it as clearly established that the uniaxal double refraction of the resinous mass has been communicated to the individual molecules by simple pressure; the increased transparency arising from the molecules being brought into closer contact, and the regular double refraction from the variable density impressed upon each elastic molecule, and symmetrically related to the axis of pressure. The effect thus produced on the resinous mass is precisely the same as what would take place by subjecting elastic spheres to a regular compressing force. The axis of pressure becomes an axis of positive double refraction, and the double refraction increases with the inclination of the ray to the axis, and becomes a maximum in the equator of the molecules.

By this view of the preceding facts, we are led to a very simple explanation of the origin and general phenomena of double refraction in regular crystals. That this property is not inherent in the molecules themselves may be easily proved. The particles of silex, for example, do not possess it in their separate state. In tabasheer, in many opals, and in melted quartz, there is not the slightest trace of the doubly refracting structure: but when the particles of silex in solution are allowed to combine, in virtue of their polarities or mutual affinities, they then instantly acquire, at the moment of their combination, the property of double refraction, and they retain it while they continue in this state of aggregation. The manner in which this takes place may be easily conceived: a number of elastic molecules existing in a state of solution, or in a state of fusion, are kept at such a distance by the fluid in the one case, and by the heat in the other, as to preclude the operation of their mutual affinities; but when, in the process of evaporation or cooling, any two molecules are brought together by the forces or polarities which produce a crystalline arrangement, and strongly adhere, they will mutually compress one another, and each will have an axis of double refraction in the directions of the line joining their centres, in the same manner as if they had been compressed by an external force.

From the phenomena of crystallization and cleavage, it is obvious that the molecules of crystals have several axes of attraction, or lines along which they

are most powerfully attracted, and in the direction of which they cohere with different degrees of force. Guided by the indications of hemitrope forms, and supposing the molecules to be spherical or spheroidal, we infer that their axes are three in number and at right angles to each other, and are related in position to the geometrical axis of the primitive form. In like manner the phenomena of double refraction are related to the same axis of the primitive form, and may be all rigorously calculated by a reference to three rectangular axes. In uniaxal crystals, the three axes A, B, C must be such that two of them are equal and of the same name; while the third, corresponding with the apparent axis, may be of the same or of a different name. In biaxal crystals, the three axes A, B, C are unequal, and in crystals with no double refraction the axes are equal and destroy each other*.

This approximation of these two classes of facts is too remarkable to be accidental, and would go far to establish their dependence, even if it were not indicated by other arguments which I shall proceed to illustrate.

Among those crystals which have the obtuse rhomboid for their primitive form, there are many with one axis of negative double refraction, and only one or two with one axis of positive double refraction. In the former, the negative doubly refracting structure will be produced round the axis of the rhombohedron by the compression arising from attractions in the direction of two equal rectangular axes A, B, which will dilate the molecules in the direction of the third axis C, and make it a negative axis of double refraction, equal in intensity to either of the other two. Here we require the combination only of two axes; but if we suppose that there is in the direction of C a third axis of attraction either more or less powerful than the other two, then if it is less powerful, the compression of the molecules produced by it will diminish the dilatation arising from the united action of A and B, but will still leave an unbalanced dilatation, or a single negative axis of double refraction in the axis of the rhomb.

If C, on the contrary, is an axis in which the attractive force of the mole-

* In uniaxal crystals, the resultant of the two equal axes A, B may have any relation to C but that of equality; excepting when C is of a different name from A and B.

In biaxal crystals, any two axes A, B, may be converted into three A + C, B ± C, ± C. See Phil. Trans. 1818.

cules is greater than along A and B, the compression which it produces will exceed the dilatation arising from A and B, and we shall have an axis of compression along C, or an axis of positive double refraction as in quartz and dioptase*. The same observations are applicable to minerals that crystallize in the pyramidal form.

When the three axes A, B, C are all equal, the three rectangular compressions, produced by the aggregation of the molecules, will destroy one another at every point of the molecule, and the body which they compose will have no double refraction, and cleavages of equal facility. Hence all crystals in which it is known by cleavage that the particles cohere with equal force in three rectangular directions have actually no double refraction.

If the three attractive axes A, B, C are all unequal, the difference of density which they produce in the molecules will be related to two axes of double refraction, the strongest of which will be negative or positive according as the compression along C is less or greater than the dilatation produced along C by the united compressions of A and B. Hence all crystals belonging to the prismatic system, in which we are informed by cleavage that the particles cohere with unequal forces in three directions, have invariably two, or, as we have already explained, three unequal axes of double refraction, of which the strongest is sometimes positive and sometimes negative.

We have supposed the elementary molecules of bodies to be spherical when existing singly, or beyond the sphere of their mutual action; but although their form must, in the case of doubly refracting crystals, be changed into oblate, prolate or compound spheroids, yet the deviation of these spheroids from the sphere may be so small, that the forms of the bodies which they compose may be regarded as arising from the union of spherical molecules. It is more probable, however, that the form of the molecules suffers a considerable change, and we may consider that change as determining the exact primitive form of the crystal and the inclination of its planes.

The circumstance of almost all rhombohedral crystals having negative

* Since this paper was written, I have seen the very valuable researches of M. Savart on the structure of crystallized bodies as developed by sonorous vibrations. The curious result of his experiments, that the axis of calcareous spar, a negative axis of double refraction, is the axis of least elasticity, while the axis of quartz, an axis of positive double refraction, is the axis of greatest elasticity, harmonizes in a remarkable manner with the above views.

double refraction, which can only be produced by axes of compression in the equator of a prolate spheroid, excludes the supposition, that the ultimate molecules are spherical particles converted by the forces which unite them into those oblate and prolate spheroids, by means of which, according to the views of HUYGENS, all the varieties of rhombohedrons may be formed*; for if this were the case, the obtuse rhombohedrons should possess one positive axis, and the acute ones one negative axis of double refraction. We are constrained therefore to suppose that in rhombohedral crystals the molecules have the form of an oblate spheroid, with its axes so related, that the change superinduced upon it by the forces of aggregation determines the exact form of the combination. In carbonate of lime for example, where the precise inclination of the faces of the rhombohedron can be produced only by oblate spheroids whose polar is to their equatorial axis as 1 to 2.8204, we may suppose that the spheroids were originally more oblate, and that the forces by which they receive the doubly refracting structure dilated them in the direction of the smaller axis, so as to produce a spheroid having its axis as 1 to 2.8204. Hence if we could suppose the molecules placed together without any forces which would alter their form, they would compose a rhombohedron with a greater angle and having no double refraction. But when they are combined by the attractive forces of crystallization, they compose a rhombohedron of 105°, possessing negative double refraction.

In this view of the subject, the form of the ultimate molecules of crystals existing separately, may be regarded as determining within certain limits the primitive form to which they belong; while the doubly refracting structure and the precise form of the crystal are simultaneously produced by the action of the forces of aggregation.

These views receive a remarkable illustration from a new doubly refracting structure, which I discovered many years ago in chabasie, and which will form the subject of a separate communication. In certain specimens of this mineral, the molecules compose a regular central crystal, developing the phenomena of regular double refraction; but in consequence of some change in the state of the solution, the molecules not only begin to form a hemitrope crystal

* See HUYGENS's Traité de la Lumière, chap. v. and the Edinburgh Journal of Science, No. xviii. pp. 311, 314.

on all the sides of the central nucleus, but each successive stratum has an inferior doubly refracting force till it wholly disappears. Beyond this limit it reappears with an opposite character, and gradually increases till the crystal is complete. In this case the relative intensities of the axes or poles from which the forces of aggregation emanate, have been gradually changed, probably by the introduction of some minute matter, which chemical analysis may be unable to detect. If we suppose these axes to be three, and the foreign particles to be introduced, so as to weaken the force of aggregation of the greater axis, then the doubly refracting force will gradually diminish with the intensity of this axis, till it disappears, when the three axes are reduced to equality. By continuing to diminish the force of the third axis, the doubly refracting force will reappear with an opposite character, exactly as it does in the chabasie under consideration.

From the mutual dependence of the forces of aggregation and double refraction, it is easy to understand the influence which heat produces on the doubly refracting structure, as exhibited in the phenomena discovered by M. Mitscherlich in sulphate of lime and calcareous spar, and in those which I detected in glauberite*. This eminent philosopher has found, by direct experiment, that heat expands a rhomb of calcareous spar in the direction of its axis, and contracts it in directions at right angles to that axis†; that the rhomb thus becomes less obtuse, approaching to the cubical forms which have three equal axes, and that its double refraction diminishes. All these effects are the necessary consequences of the preceding views. The expansion in the direction of the axis, and the contraction of all the equatorial diameters diminish the compression of the axes of the oblate spheroidal molecules, and must therefore diminish its double refraction, as well as the inclination of the faces of the rhomb. In

* See Edinburgh Transactions, vol. xi.

† It follows from this fact, that massive carbonate of lime, in which the axes of the molecules have every possible direction, should neither expand nor contract by heat, and would therefore form an invariable pendulum. As there must be, in any given length of massive carbonate of lime, as many expanding as there are contracting axes, then, if the contractions and expansions in each individual crystal are equal, they will destroy one another; but if they are proportional to their lengths, the contractions will exceed the dilatations. In this case, we have only to combine the marble with an ordinary expanding substance, to have an invariable pendulum. The balances of chronometers might be thus made of mineral bodies.

like manner it will be found that in sulphate of lime and glauberite the expansions and contractions will be so related to the three axes, as to explain the conversion of the biaxal into the uniaxal structure, and the subsequent reappearance of the biaxal structure in a plane at right angles to that in which the axes are found at ordinary temperatures.

The phenomena exhibited by fluids under the influence of heat and pressure, and those of doubly refracting crystals, exposed to compressing or dilating forces, are in perfect conformity with the above views; so that even without the fundamental experiment described in this paper, we might have been entitled to conclude that the forces of double refraction are not resident in the molecules themselves, but are the immediate result of those mechanical forces by which these molecules constitute solid bodies.

Allerly, October 5th, 1829.

VI. *Experiments on the influence of the Aurora Borealis on the magnetic needle. By the Reverend* JAMES FARQUHARSON, *F.R.S. Minister of Alford, Aberdeenshire. In letters addressed to Captain* EDWARD SABINE, *Secretary of the Royal Society.*

Read January 28, March 4, and April 1, 1830.

Alford, December 15th, 1829.—THE apparatus, belonging to the Royal Society, with which these experiments were made, consists of a horizontal brass circle, about one foot in diameter, graduated to divisions of 10 minutes, and capable of adjustment to a perfect level by means of spirit levels and screwed feet. Concentrically within this divided circle moves a circular horizontal brass plate, its edge touching the divisions, and having at opposite points two verniers, which, by means of attached microscopes, indicate the movements which it makes to 60th parts of 10 minutes, or 10″. The movement of the plate within the circle is effected by means of a screw. A circular brass needle-box is attached to the surface of the inner plate, and a vertical pointed steel wire for supporting the needle forms the centre. At opposite points in the needle-box are fixed two micrometers with cross wires in the foci, for adjusting the needle to a level, and observing any change in its direction. The top of the needle-box is a circular plate of ground glass in a brass ring, made to slip easily off and on, and having screwed into its centre a vertical brass tube about 8 inches long, for the purpose of suspending the needle with fibres of silk, for measuring the time of its oscillations. A horizontal brass pin, with a minute perforation for the silk near its middle, passes through the vertical tube near its top, and being contrived with several motions, serves to adjust the suspended needle, and bring it correctly over the steel point, where its levelling can be completely ascertained.

The magnetic needlec itself is a rectangular plate about 5 inches long, half an inch broad, and $\frac{1}{20}$th of an inch thick. An agate cup set in brass admits of being screwed in either at the narrow or flat side of the needle; and a little fixt ring of brass, with a minute perforation in its top, rising over the cup,

admits of the ready attachment of the silk; so that the needle can be placed on the steel point or suspended with the silk, with its flat face either vertical or horizontal.

This apparatus measures, with great accuracy, very minute changes in the declination of the needle. A change as small as 10″ is quite sensible by it. It was placed on a firm-set table, in a room of my house, on the 21st of September last; and by shifting the whole apparatus, the cross wires of the micrometers were brought into a line with the needle, when at rest at 8 o'clock in the evening; the index of the vernier being at the same time brought to zero on the divided circle. The readings of the variations of the needle are therefore reckoned from its position at that hour, which was made choice of as being that when the Aurora borealis was most likely to appear, and thus the diurnal variation would be brought to interfere as little as possible with any variation induced by that meteor.

The needle, where it is stationed, is subjected to some influence from several fixt pieces of iron, but not to any from iron that is moved from place to place. There was a necessity for removing the apparatus temporarily from its station on the 24th of November; but it was carefully replaced on the 2nd of December; marks having been made in the floor for the feet of the table, and on the table for the feet of the apparatus; and the cross wires brought to the line of the needle at 8 P. M. as before.

In making observations on the intensity, the time occupied by a certain number of horizontal vibrations of the suspended needle is measured by a stop-watch, the character of which it is necessary to describe. It is a time-keeper, on which, indeed, not much reliance ought to be placed, if it were necessary to have the intervals estimated in absolute mean time; as, although it is adjusted to return nearly to mean time at the end of every twenty-four hours, when only wound up once during that period, yet it goes very unsteadily at many of the intermediate hours. I have found, however, that it keeps time nearly with a well-regulated pendulum clock, from 12 minutes after it is wound up till about an hour after that time. It is therefore always prepared for the observations by being stopped 12 minutes after it is wound up; and thus, although the intensities measured by it could not very safely be compared with those measured by a more correct watch, yet considerable reliance may be

placed on the comparison of measures thus made by itself at different times. I have in fact already discovered, by using these precautions, such an increase of intensity as was to be anticipated during the cold season now come on. My observations have been too desultory, owing to numerous other avocations, to permit me to look out for a diurnal change of intensity.

The balance of the watch makes 290 vibrations in a minute, so that the vibrations are not commensurate to the seconds; recourse therefore must be had to reading off the time by approximation on the seconds dial-plate. The time is estimated to the nearest quarter of a second by means of a microscope, and the watch relieved from the point where it stops for a new observation; thus neutralizing any incorrectness in the reading off, and any inequality in the divisions of the dial-plate on the principle of the repeating circle. The stop is on the balance of the watch, and is therefore instantaneous.

After trials of various arcs, I have found it most convenient, for insuring that the different series of observations shall commence in equal arcs, to make the needle vibrate by its own breadth at the extremity, that is, in arcs of nearly 12°, which is correctly determined when the corners of the needle come alternately to the cross wires of the micrometer. The watch is relieved the instant the centre of the needle comes to the cross wires the third time after the extent of the arc has been noted by the corners; 50 oscillations are then reckoned, and the watch stopped for reading off the time. A very small piece of iron is employed for moving the needle, which is instantly deposited at some distance at right angles to it; and for night observations my candlestick is of glass.

I had no leisure for making observations on the intensity for several days previous to the 14th of December. The last I made were on the 2nd, 3rd, and 4th of December,—4 sets of 50 oscillations each day at 8 P. M.; average of each set, and of the whole, in 225″.25: Therm. from 35° to 42°.

I shall now copy from my journal the observations of the 14th of December. They were set down at the time, at intervals of a quarter of an hour.

5¾ P. M.—Aurora seen in various quarters of the sky.

6 P. M.— Arch of nebulous light in N. about 25° high; another S. about 30° high; vertex of each about the mag. mer.: S. arch just over a continued line of clouds. Many detached clouds in region of N. arch.

Sky between these quite clear of clouds, with many brilliant streamers of Aurora, forming evidently parts of two fringes coming towards the zenith from N. Bar. 30°, Therm. 39° ; nearly calm.

6½ P. M.—Many groups of very brilliant short streamers now passing into the zenith. Needle on steel point 2′ W. Intensity, 1st set of 50 oscill. in 225″.25 ; 2nd in 225″.25 ; 3rd in 225″.

6¼.—Narrow arch near the zenith, very faint, and small clouds appearing there. Arch in the S. descending to the horizon, and clouds descending with it. That in the N. now about 35° high, accompanied by many clouds. Needle on steel point shifted to 5′ E.

6¾.—Needle on steel point now 23′ E. S. arch now extinct. N. one about 45° high ; nebulous lights under it, and clouds rising with it.

7 P. M.—Needle on steel point slowly returning westward.

7¼.—Needle at zero.

7½.—Needle 4′ W. Aurora still forming an arch at N. ; but not advancing higher than 45°.

7¾.—Needle still 4′ W. Several arches of nebulous light in N. under 45°, with many clouds ; gentle gale a point or two S. of W.

8 P. M.—Arch of nebulous light again formed at S. about 45° high. W. and middle parts of N. arches expiring as they reach 45° ; but their E. ends going now much further S. and passing the prime vertical to the mag. meridian ; not fully seen, however, for clouds, which are following the Aurora in all its flittings ; the rest of the heavens being quite clear. Needle on steel point now 7′ 50″ E. Intensity, 1st in 225″.25 ; 2nd in 225″.25. Therm. 38°.

8¼.—Needle on steel point 13′ 40″ E.

8½.—Needle on steel point 8′ 50″ E. Intensity, 1st in 225″.25 ; 2nd in 225″.25. Nebulous light at N.W. and very brilliant groups of streamers at N.E. Aurora in the S. now extinct.

8¾.—Aurora fading ; needle on steel point 4′ 20″ E.

9 P. M.—Intensity, 1st in 225″.25 ; 2nd in 225″.25. Needle on steel point 2′ E.

9¼.—Three arches of Aurora of unusual brilliancy at mag. N. coming up rapidly with most splendid streamers. Fragments of the uppermost soon passed the zenith, and needle shifted slowly to 19′ W.

9½ P. M.—Very obscure narrow nebulous arches across the zenith, and a little S. of it at right angles to mag. mer.; being the remains of some of the arches seen at 9¼. Needle shifted to 9′ 50″ E.

9¼.—Only a few streamers near N. mag. mer. 25° high. Needle now 12′ E.

10 P. M.—Very brilliant arch of streamers in N. about 25° high; and narrow obscure arch across mag. mer. a little S. of zenith. Needle 14′ E. Intensity, 1st in 225″.25; 2nd in 225″.25. Therm. 37°. No clouds.

10¼.—Arch of streamers in N. about 25° high, not very brilliant. Many groups of faint streamers higher up; and nebulous narrow belts of pale light across the zenith and southward of it a little, at right angles to mag. mer. These are all expiring as they go S. in succession. Needle shifted as I was watching it from 14′ E. to 3′ W. in the space of about 6 minutes.

10½.—Many groups of very pale streamers over all the northern half of the sky, and the western half of a very pale zenith arch has passed to about 20° S. of the zenith. Needle now 21′ 30″ W. Intensity, 1st in 225″.5; 2nd in 225″.75. Therm. 35°.

10¾.—Aurora nearly faded every where, and heavy clouds forming in N.W. Needle returned to 3′ W.

11 P. M.—Aurora extinct. Needle at 5′ W.

12 P. M. Nebulous light at the horizon in mag. N. has continued for nearly an hour, fading at 10° high, and rising again from the hills. Needle 1′ E. and has remained so for three quarters of an hour. Many falling stars whose paths are parallel to the lines of the streamers, or at right angles to them in the planes of the fringes. Intensity, 1st in 225″.5; 2nd in 225″.25. Breeze a point or two S. of W.

December 15, 1 P. M.—Intensity, 1st in 225″.25; 2nd in 225″.5. Therm. 34°.

It ought to have been stated, that from noon on the 10th of December till the evening of the 14th, there was a continued series of the hardest gales at S. W. and W. that have occurred this autumn.

It is obvious from the above detail, that the disturbance of the magnetic declination by the northern lights on the evening of the 14th of December was so great, and so frequently reversed from E. to W., and the contrary, as to leave no shadow of doubt regarding the reality of the phenomenon. It is to be ob-

served, however, that no disturbance took place till the fringes of the Aurora had gone so far south as to place the needle in their planes, or in the line of prolongation of the middle or most elevated streamers of the fringes, which is that of the dipping needle. The numerous observations formerly made by me this autumn, of which I have already put you in possession*, and those also

* 10th of October.—11 A. M. Series of 50 oscillations: 1st in 227″.75; 2nd in 227″.5; 3rd in 227″.2; 4th in 227″.5; 5th in 227″.75; 6th in 227″.5.

2 P. M.—1st in 227″.5; 2nd in 227″.5.

4 P. M.—1st in 227″.75; 2nd in 227″.5.

11th of October.—6½ P. M.—Very brilliant streamers seen, at an opening among clouds at N. W. reaching upwards to about 35°. Series of 50 oscillations: 1st in 227″.75; 2nd in 227″.5; 3rd in 227″.5; 4th in 227″.75.

7½ P. M.—Column of very brilliant light near the horizon at the W. extremity of the prime vertical to the mag. mer. and arches of Aurora in the N. Series of 50 oscillations: 1st in 227″.5; 2nd in 227″.75; 3rd in 227″.5; 4th in 227″.25.

It is evident that the differences between the averages of these series at the several periods, are within the limits of the probable errors of observation; so that the intensity was not sensibly affected by the Aurora.

At 8 P. M. the needle on the steel point remained steady at its usual position at that hour.

At 20 minutes past 8, needle on the steel point was still steady, with many brilliant groups of streamers in the N. and an arch of light below them, and another bright column at mag. W.

On the 25th of October the Aurora again appeared; but neither at this time could I find that either the intensity or direction of the needle were altered.

17th of November.—3 P. M. 50 oscillations: 1st in 225″.75; 2nd in 225″.5; 3rd in 226″.25; 4th in 226″.

6 ¼ P. M.—A complete arch of pale nebulous light, with its vertex about 20° high at mag. mer. Intensity, 1st in 225″.25; 2nd in 225″.25. Needle on steel point steady at its usual place at 8 P. M.: and intensity, 1st in 225″.5; 2nd in 225″.5.

A succession of arches rose one below another, and successively expired at about an elevation of 20° till 11 P. M. when the western end of one of them became unusually brilliant, and dense. Streamers. Intensity at this hour, 1st in 225″.5; 2nd in 225″.75; 3rd in 225″.5. Needle has now 3′ E. var. from its position at 8 P. M. At 12 P. M. Intensity 1st in 225″.25; 2nd in 225″ 25. Var. 3′ E.

On the 18th of November, Aurora first seen at 6 P. M. At 8 P. M. with a very brilliant arch about 20° high, and a few streamers, the remains of a preceding one, about 35° high. Intensity, 1st in 225″.25; 2nd in 225″; 3rd in 225′.5; 4th in 225″.5. Variation at 8 P. M. 2′ E.—At 9½ P. M.; Intensity 1st in 225″.25; 2nd in 225″.—At 10 P. M. Var. 4′ E.—At 11½, Var. 3′ 20″ E.

19th of November.—At 2 P. M.; Intensity observed, 1st in 225″; 2nd in 225″; 3rd in 225″; 4th in 225″.25.

At 8 P. M. Aurora first seen. A pale light through many clouds about 15° high. Variation not altered. Intensity, 1st in 225″; 2nd in 225″.25.

[The variations at 11 and 12 P. M. of the 17th of November, and at 8, 10, and 11½ P. M. of the 18th of November, were within the limits of the irregularities of the diurnal variation.]

now detailed, afford a proof,—a negative one indeed, and therefore imperfect, but now very extensive,—that the needle is not affected by the Aurora, till it comes into the plane of the dip: and we have thus, I trust, an explanation of the discordant results obtained and announced regarding this matter by different former observers. When the disturbance has been observed, there have been no doubt zones of Aurora in the plane of the dip, though perhaps obscure and liable to be overlooked. Most of those I observed in that position on the 14th were so faint, that I might not have noticed some of them, had I not traced their progress southward from stations where they were much more brilliant.

It would be as yet premature to infer any connection between the easting and westing of the declination, and the vividness of the fringes in their eastern or western ends. What occurred at 8 P. M. and ½ past 10 P. M. might lead us to suspect that the needle declines towards the most vivid end.

In regard to the intensity, I cannot venture to say, that the slight decrease indicated at ½ past 10 is not an error of observation; being conscious that the limits of errors of this kind ought not to be taken at less than the change then noted. I could not take the intensity at ¾ past 6, when the declination was greatest, from a desire to watch the extent of the change in declination produced by the Aurora, of which I was for the first time fully assured. When I receive the separate apparatus for the intensity, with which I am to be favoured, I shall expect more certain results on that point.

Alford, December 26th, 1829.—Since I addressed you on the 15th current, I have made some additional observations on the Aurora borealis, of which I proceed to put you in possession in continuation.

From Journal of observations.

Frost during the 16th, 17th, and 18th Dec. Therm. at night 19°.

19th Dec.—10 P. M. Bar. 29°.7. Therm. 30°.—At ½ past 11 P. M. a heavy low fog cleared up, and exposed a very brilliant Aurora. An arch of very brilliant streamers about 25° high over dense clouds in the N. A broad lane of clear sky above these from W. to N. E. Over head many broken clouds giving partial views of a narrow arch of Aurora from E. to W. in the plane of the mag. dip, and many streamers approaching the zenith from N. To the S.

of these another clear lane of sky E. and W. and nebulous Aurora over dense clouds at S. horizon. Needle on steel point 36′ 20″ E. Before the watch was prepared for the intensity, needle shifted to only 33′ E. Intensity then found 50 oscillations in 226″.25. Needle at conclusion of the observation 14′ E. —12 P. M. Another arch getting into plane of the dip, the rest of the Aurora presenting nearly the same appearances as before. Needle shifted to 22′ E. Intensity 50 oscil. 1st in 225″.5 ; 2nd in 225″.5. Therm. still 30°. Needle after observation still 22′ E. Steady slight wind near S. W.

20th Dec.—From ¼ past 8 P. M. till 11 P. M., a very splendid Aurora continued, with periodical obscurations and revivals, over a dense cloud resting on, and capping the summits of the North hills. Aurora never getting higher than 20°. Rest of the sky entirely clear of clouds, and slight steady wind at W. Therm. 25°. Needle not affected.

21st Dec.—8 P. M. Intensity 225″. Therm. 27°. Ground covered with snow.

22nd Dec.—I have been informed, this day, by the Rev. James Paull, minister of Tullynessle, that on the 20th of Dec. about ¼ past 9 P. M. he saw the Aurora remarkably bright, near the zenith, at his house, about 2 miles N. of this place, and I am permitted to use his name for the fact.—8 P. M. Intensity 224″.75 ; 225″. Therm. 28°.

23rd Dec.—8 P. M. Intensity 225″. Therm. 30°.

24th Dec.—8 P. M. Intensity 225″. Therm. 30°.

25th Dec.—8 P. M. Intensity 224″.5 ; 224″.75 ; 224″.75. Therm. 29°.

I hope the Royal Society will find in the above observations, and in those I transmitted to you on the 15th, a settlement of the much agitated question regarding the disturbance of the magnetic needle by the Aurora borealis. Mr. Dalton was the first, as far as I am acquainted, who observed that the streamers direct their upper extremities to that point in the heavens to which the dipping needle is directed, and that the arches they form are at right angles to the magnetic meridian ; but the definite order in the southward progress of the fringes of streamers not having been then ascertained, those circumstances under which alone it now appears that the magnetic needle is disturbed, were for a time overlooked ; and results apparently the most opposite were announced by different observers of distinguished skill and reputation.

A proper value will now be put on the results obtained by Captain Franklin and Lieutenant Hood, chiefly at Fort Enterprise. They observed a remarkable disturbance of the needle when the Aurora passed the zenith; that is, when the fringes came into the plane of the dip, which is there 86° 59′. But the full admission of this fact does not invalidate the results of an apparently opposite nature obtained by others, or their importance. The observations I made on the 21st, 22nd, and 26th of September, 1st, 3rd, 11th, and 25th of October, and the 17th, 18th, and 19th of November last, and now of the 20th December, show, that with very brilliant Aurora, there is no disturbance of the needle, if the fringes do not come into the plane of the dip.

It is evident that as the needle is affected in those places only where the fringes are in that plane, observers in different latitudes may obtain very discordant results on the same evening. The numerous observations collected by Mr. Dalton, of the appearances of Aurora on the 29th March, 1826, (Phil. Trans. 1828,) prove that many fringes of streamers may be parallel to each other at remote distances; and the observation, by the President of the Royal Society, of a luminous arch in Cornwall, 29th September, 1828, simultaneously with a remarkable Aurora of many arches over the whole of Aberdeenshire, proves that the meteor is sometimes active over a space nearly coincident with the extent of this kingdom; and we have no reason to suppose it may not extend often much further. There might therefore be an extensive succession of observations of disturbance and non-disturbance of the needle, at the same instant, from N. to S., over many degrees of latitude. Next to the discovery of the truth, it will give me the highest satisfaction if the Royal Society shall be of opinion, that the result of the observations they have enabled me to make, is to reconcile the conflicting statements made regarding this matter, leaving unimpeached the accuracy of all the observers.

The observations of the intensity, on the 19th December, lead now more plainly to the inference, that it is decreased under the influence of an arch of Aurora in the plane of the dip; but the indications of decrease are too small to be fully confided in, until after a long series of similar results.

I consider Mr. Paull's observation on the evening of the 20th December, viewed in conjunction with mine, of great importance in various respects. The same Aurora that I saw here under an angle not exceeding 20° high, he saw at

the Manse of Tullynessle very near the zenith. Now the Manse of Tullynessle is about one point E. of due N., distant from this place little more than two miles in a direct line. It is in a narrow side valley that enters northward into a ridge of hills, the nearest summits of which are nearly two miles from here, about the N. point of the magnetic meridian. These are the summits that were capped by the clouds, to the region of which the Aurora was confined the whole evening. A line drawn over them at mag. E. and W. would pass very nearly over Mr. PAULL's station. The height therefore of this particular Aurora at its upper extremities, did not exceed 4000 feet above the level of this place.

In my paper on the Aurora borealis, which the Royal Society honoured with a place in their Transactions of 1829, I stated "that I have here seen the meteor much more frequently in the form of a light near the northern horizon, than in any other form." Mr. PAULL's observation has convinced me, that the true place of this light near the horizon is often no other than the nearest mass of hills to the north.

If any doubt had yet remained regarding the height of the region occupied by the meteor, this observation would have determined the point. The merit of first accurately ascertaining that region is due to Lieutenant HOOD and Dr. RICHARDSON, by their observations at Cumberland House and Basquiaw Hill. It is the region immediately above that of the clouds, and of course varies much in height with different states of the atmosphere. Although this region was very low on the 20th December, it is, we know, at times several miles high, agreeing with the observations of those distinguished travellers. I have seen the Aurora here when the height of the clouds could not be estimated at less than two or three miles; and I state this again more precisely, because I understand what I had formerly said, has, by some persons, been misunderstood to imply, that its height never exceeds six or eight thousand feet.

Captain FRANKLIN and Dr. RICHARDSON first observed also the connection of the Aurora with the formation of clouds. The latter even says, "I am inclined to infer that the Aurora borealis is constantly accompanied by, or immediately precedes, the formation of one or other of the various forms of cirro-stratus." He could not however determine whether the Aurora was dependent on the formation of the cloud, or the formation of the cloud on it. I conceive the

observations of the 20th December throw a satisfactory light on this subject, and shall now briefly state the conditions of the phenomena of that evening, and their localities, which lead me to this conclusion.

The mass of hills, named Coreen, to which both the clouds and the Aurora of that evening were entirely confined, is eight or ten miles long from E. to W., and about four miles broad in some places from N. to S. It is nearly bisected by the magnetic meridian of this place, which cuts off the broadest and highest part of it to the W. About twelve or fifteen square miles rise above the limits of cultivation; that is, about six or seven hundred feet above the bottoms of the conterminous valleys; and some of the summits attain an elevation of nine hundred and one thousand feet.

On the W., a little by S., of this mass of hills, there extends for eighteen or twenty miles a succession of valleys of considerable width, all bounded on the S. and N. by high lands of great extent, and separated from each other by comparatively low eminences. Through these the river Don flows from W. to E. On descending this succession of valleys with the course of the river, the Coreen hills are seen directly in the line of them, and apparently shutting them nearly up at the E., the extension of the low ground turning there, for some distance, suddenly N. and afterwards E., by the N. side of these hills; and the river, on the contrary, when it reaches these hills, turning several points S. of its former course, and entering this valley of Alford at its N. W. corner, by a very narrow defile between Coreen and another hill on the S., of nearly equal elevation but comparatively little extent, which is placed more in the direction of the general outline of the high lands bounding the valleys on the south.

We have then, in the situation of the Coreen hills, a most satisfactory explanation of the formation of clouds over them on the 20th December, in well established principles independently of the Aurora. The wind coming steadily the whole evening from W. carried the air out of the upper Don valleys, (where, at a comparatively low elevation, it was nearly saturated with aqueous vapour, to which the river in its long course would much contribute,) over the surface of Coreen; that is, lifted the whole mass several hundred feet perpendicularly, and the diminished pressure and consequent expansion lowered its temperature to the point of saturation.

As the Aurora was entirely confined to the region of the cloud, the cause of

the formation of which we can thus otherwise clearly trace, we are surely justified in considering it as an effect of the condensation of vapour; and as the evaporation and condensation of the aqueous fluid are ascertained agents in developing electricity, the meteor must be considered as a peculiar manifestation of the electric fluid *.

The parallelism of the streamers with the dipping needle; the position of the fringes at right angles to the magnetic meridian; the movement of these fringes away from the N. magnetic pole, and their effect on the needle when they come into the plane of the dip,—all prove it to be equally a magnetic phenomenon; so that we here find another relation between the electric and magnetic influences.

I conceive the result of these observations coincides with many discoveries of a very recent period, to show that we are on the point of being compelled to resolve the long received theory of the magnetic action of the nucleus of the earth, into a peculiar influence of its atmosphere and superficial electricity.

Alford, February 11th, 1830.—Between January 10th and 20th, several series of observations of the intensity of the needle were taken at 8 P. M. Therm. uniformly near 30°; and 50 vibr. very uniformly in 225″.25. [It thus appears that the annual increase of the intensity was greatest in the end of December.]

January 25th.—No Aurora was seen since 20th December till this evening, when for about 3 hours from 7 P. M. there were seen, over many detached clouds resting on the Coreen hills in the N., several low arches and parts of arches of brilliant streamers, rising in succession below one another, and expiring about 20° high. Rest of the sky quite clear. Therm. 30°. Needle not affected. Many falling stars in paths parallel to streamers. None of these meteors were seen during the absence of the Aurora.

January 26th.—A steady gale all day a point or two S. of W. Therm. 34° and 35°. Gale became hard after dark, and much snow melted before next morning. The ground has been covered with snow, of which there have been

* This is in accordance with Mr. DALTON's conclusions, that electricity appears to be a consequent rather than an agent in the formation and decomposition of clouds. (Observations on Meteorology, &c. Manchester Memoirs, Second Series, vol. iv.)

many falls since the 20th December, with a nearly steady frost, generally from 30° to 35° Fahr., but once or twice as low as 12° at sunrise. Wind since that time till now in all points, except W. and S. W.

January 27th.—Again frost and calm. Therm. 30°. Intensity 225″.25.

January 28th, 8 P. M.—Several low arches of Aurora of great brilliancy, with many streamers over clouds resting on the Coreen hills. Rest of the sky quite clear. Needle steady. Therm. 25°.

½ past 8.—An arch has come slowly forward and passed the zenith into the plane of the dip; its west part fading as it reaches that position; but the east part from the mag. merid. continuing moderately bright. Needle shifted to 21′ 30″ E. or towards the bright part. Intensity 225″.25. After this observation, needle on steel point returned slowly to 6′ E.

5 min. before 9.—Another very bright arch, about 3° broad at the vertex, and extending nearly from the E. to the W. horizon, but narrowing much at its extremities, came forward into the plane of the dip. The light of this very beautiful arch was nearly uniform from end to end. Sky quite clear, when it was in the plane of the dip, excepting over the Coreen hills, where there were still many clouds. Needle on steel point moved incessantly at this time, but irregularly and somewhat fitfully, alternately to E. and W. within the limits of about 30′ in all. Intensity 226″. Arch faded slowly about 30° S. of the zenith; and needle on steel point returned to rest nearly at zero.

Many falling stars in paths parallel to the streamers, i. e. to pencils of rays of the Aurora, were they at the same places.

Many low arches rose in succession after this to about 25° above the Coreen hills; some of them with very brilliant streamers. Needle steady. A slight steady breeze a point or two S. of W. set in after 9 P. M. At 11 P. M. therm. 24°.

January 29th.—Therm. 36°. A steady gale all day, a point or two S. of W. till sunset, when it shifted to N., and therm. fell to 30°. 8 P. M. Intensity 225″.25.

February 6th and 7th.—Steady moderate frost till now, when there occurred a terrible storm at S.S.E. with a heavy fall of snow. At 8 P. M. of 6th, therm. 27°. Intensity 225″.5.

It will be seen that the observations now detailed confirm the result of those I formerly sent, viz., that the magnetic needle is disturbed by the Aurora only

when the fringes come into the plane of the dip. I employ the term fringe, which I first used in my paper on the definite arrangement and order of progress of the meteor, as the term arch can have reference properly to two only of its dimensions, while fringe may include the idea of all the three.

The observations of the 28th January lead to the conclusion, that the N. pole of the needle (meaning the pole that is directed towards the N.) shifts towards the most brilliant end of the fringe. The successive phenomena of that evening were remarkably distinct, giving great facility to accuracy of observation; and I remember nothing of the kind surpassing them in splendour. The first fringe of that evening, which crossed the zenith, and faded in its W. end as it got into the plane of the dip, exhibited, in the most distinct way, the manner of the progress southward, by the extinction of streamers at its northern face, and the sudden formation of new ones along the whole of its southern face.

After making the attempt, I have found it impracticable to keep any journal in detail, of the falling stars, on account of the great numbers that often occur in the same evening; but I have not failed to observe them since my attention was directed to them in the remarkable manner I mentioned to you in a former letter*. I have now no doubt whatever that they are a branch or modification of the Aurora borealis. In a vast majority of cases their paths are parallel to the streamers of the aurora, that is, they are directed away from that part of the heavens to which the upper end of the dipping needle points; and in those cases, comparatively few in number, where this parallelism

* *Date Nov.* 11.—During the course of these observations, my attention has been incidentally directed to another meteor, that of falling stars; and the conclusions to which my observations of these phenomena (as yet I must acknowledge very limited) have uniformly led, I have no doubt you will acknowledge to be interesting. I would have introduced this subject to you in my last, had I not felt it proper to wait till the views regarding them, which had at that time opened to me, should be ascertained to be correct or not, by some continuance of observation.

On the evening of the 21st of September at 10½ P. M. there occurred an Aurora, rising above the northern horizon in detached groups of bright streamers and nebulous patches of light. Many flashes of sheet lightning were seen, during and after its continuance, in the western part of its space; and in the eastern part several falling stars descending among, and having paths quite parallel to, the streamers. I could not quite satisfy myself whether the course of the sheet lightning was vertical in a downward direction, on account of its suddenness; but that was the impression to which my observations chiefly led. But the correct parallelism of the paths of the falling stars to the streamers led

is departed from, the paths can generally be referred to the planes of the fringes of the Aurora, and, within these, to lines at right angles to the streamers. I have even seen a succession of three or four or more falling stars, at momentary intervals, whose paths were all wholly within the plane of a fringe, or such a narrow zone as an elevated fringe would occupy; and again, after a considerable interval, have seen a similar succession in a similar zone, further S., as if the planes in which they are active moved southward like the fringes of the Aurora. But the circumstance which perhaps most convincingly connects them with the Aurora, is their occurring in great numbers on those evenings when it is visible, and their rareness at any other time, a fair example of both which cases is to be found in the foregoing journal.

It will be perceived from this and my former communications, that the Aurora precedes and accompanies W. and S. W. gales, which are generally the hardest we experience. In the absence of more direct observation, I may be permitted at present, although it is not strictly legitimate, to refer to the popular belief, that the falling stars also are indicatory of hard gales.

I think there is now no doubt that the horizontal intensity is affected by the Aurora borealis, as the indications are uniformly on one side, and longer practice has given me more confidence in the correctness with which I am enabled to measure the times.

me to examine the direction of those I have seen since. They are now very numerous, and, without any exception, their paths have been directed from a point near the meridian, within the limit of from 10° to 20° south of the zenith. These meteors, too, have been most frequent on those evenings when the Aurora borealis appeared, with the exception of the evening of the 16th October, when there was no Aurora, but a great number of falling stars. The state of the weather and wind that night was, however, such as led me to anticipate and make preparation for a display of the former. The paths of those falling stars I have seen near the zenith have been all short in comparison with those seen on the same night lower down.

These observations of mine have been too few, and limited to too short a period of time, to give confidence in the inferences which are to be deduced from them; but being now put into your possession, they will either soon be confirmed or their partial character shown: but it may surely be worthy of inquiry, whether the falling stars of southern latitudes are the equivalents of our Aurora borealis.

March 24th, 1830. (Journal continued.)—Therm. 45°, observed a remarkable Aurora. A bright fringe having a little deficiency near its vertex, but reaching low towards each horizon, and about 4° broad, is near the plane of the dip. To the northward of this, and crowding up to it with intervals only of about 3°, 4°, and 5°, many fragments of another fringe, some of them 10° or 20° long from E. to W., others much shorter; the general breadth of these varying from about 4° to 6° or 7°. N. of these again other fragments of a fringe or fringes, having their most lengthened dimensions very perceptibly in an E. and W. direction; and the whole northern part of the sky, down to a long dense cloud stretching the length of the Coreen hills and elevated only 8° or 10° above them, is filled with brilliant streamers, at times presenting apparently inextricable confusion, and at other times partial appearances of arches. No clouds in the region of the Aurora, nor southward of it, with the exception of one on the S.W. horizon. A bright nebulous light about 15° high along the whole S. horizon.

The whole lights in the N. part of the sky made a rapid progress southward; and the manner of this progress was repeatedly finely exhibited in the fringes and fragments that had reached or passed the zenith, by the extinction of streamers at their northern faces and the formation of new ones at their southern faces. The advanced southern fringe expired when it had reached about 25° S. of the zenith; and all did so, either when they attained a similar angle S., or before they had gone so far. The confused mass of streamers in the N. as they came forward in succession to the zenith, and passed that point, unfolded themselves into narrow zones of light at right angles to the magnetic meridian, or very nearly so; for there was occasionally a small deviation from parallelism among themselves. These zones were more numerous than I have seen on any evening before, and were separated from each other by less intervals, sometimes not exceeding 3° or 4°, sometimes however 15° or 20°. There were among them two complete and very lengthened fringes, besides the one first described; but the larger proportion consisted of only fragments of similar fringes, cut short more or less in their E. and W. dimensions.

The needle on the steel point, from the first moment of observation was very unsteady, shifting sometimes more slowly, and sometimes more rapidly and

fitfully, within the limits of about 50°, in the most extreme case, on each side of the ordinary magnetic line.

5 min. past 9 P.M.—With a very lengthened fringe in the plane of the dip, varying much in intensity of light from end to end; the needle on the steel point shifted to 32° W.; I then made trial of the intensity with the needle, and observed the arc of oscillation, at the conclusion of 50 oscillations, much less than I have uniformly before observed it. Time of 50 oscillations 224″. Dropt the needle on steel point, and found it now (a few seconds only after concluding the observation) 25′ E. Repeated the trial of the intensity immediately, and again found the arc of oscillation at the conclusion greatly diminished. 50 oscillations in 224″.25. Needle instantly dropt on the steel point now returned to 34′ W.

Having examined the needle on the steel point for some considerable time, and the extremely irregular nature of its movements, under the influence of succeeding fringes coming into the plane of the dip, I conceive the true explanation of the irregularity in the extent of the arcs occurred to me. The arc of oscillation being bisected by the line of the magnetic force, when this line changes its direction suddenly, as is indicated by the needle on the steel point, the arc is either enlarged or diminished suddenly at the same time, according as the line shifts from or towards that of the needle, at the moment when it has completed an oscillation, to return in another; and the time of the oscillations is thus sensibly changed.

This conclusion might indeed have been drawn from the previously observed phenomena, but did not occur to me till the experiments of this evening brought it into view.

Besides changes in declination, the needle on the steel point often exhibited a slight vertical movement and tremor, indicative of a change of the dip, and showing thus another impediment to an accurate determination of the intensity.

At a quarter to 10 P.M. the region occupied by the Aurora was extensively obscured by a haze, preventing further good observation.

Several circumstances occurred this evening confirming the views that the observations of the 20th December last had led to; viz. "That the Aurora is dependent on the condensation of vapour." It is familiar to every one, resident in a mountainous country, that clouds, even during hard gales, continue appa-

rently stationary over the higher summits, and present themselves in these situations when the rest of the sky is clear. The explanation is quite simple. The lower saturated strata of air being carried over the summits, the diminished pressure and consequent expansion lower the temperature to the point of condensation; and as the air in its progress again descends, a reverse effect occurs, and the vapour is again dissolved. A cloud of this kind appeared to rest the whole time of the appearance of the Aurora over the line of the Coreen hills, little elevated above them, and appearing to furnish, as it were, the base of the bright Aurora in the northern part of the sky.

It has been stated that a detached cloud rested on the S. W. horizon; and the description of the circumstances attending it has been deferred till now. It continued apparently stationary all the time of the observations from 20 minutes to 9 till a quarter to 10; and I had no difficulty in referring it with certainty to the summit of a large hill about six miles from this place, having considerably more elevation than the Alford hills, and standing several miles apart from any summit of nearly equal height. Now over the top of this cloud, which was quite free of the northern Aurora, and also of the nebulous Aurora at the S. horizon, there continued the whole time a very bright nebulous light. The large hill referred to is not itself visible from this place, on account of a high ground about a mile distant.

I have little doubt also that the nebulous Aurora at the S. horizon, which likewise continued the whole time, ought to be referred to a range of hills extending E. and W., about seven miles distant, and nearly as high as the Coreen hills. A high ground about three quarters of a mile distant conceals these hills from view, and would also conceal clouds over them, as high as those above the Coreen, on this evening.

It would thus appear that the pencils or bundles of parallel rays of the Aurora, denominated streamers, and whose longitudinal dimensions always show themselves parallel to the dipping needle, become indistinct at the distance of a few miles, being blended and softened down into a nebulous light by the refraction and haze of the atmosphere.

The wind the whole time was a rather strong gale a point or two S. of W. There have been very severe gales from the same quarter for three days past.

I have this evening again seen several falling stars during the continu-

ance of the Aurora, having seen none since I saw it last. Four were over the Coreen hills, and the paths of all were nearly but not quite parallel to the streamers. As if in correspondence with the great brilliancy of the Aurora, they were very large and bright, fully equal at least to Jupiter.

My attention having been sometimes directed to the diurnal variation of the needle, for the purpose of ascertaining whether any interference might be expected between it and the influence of the Aurora, I have been led by many observations (of the whole of which indeed I have no regular notes, but only of a few,) to conclude that the quantity of the variation depends on the brightness or gloominess of the weather. Thus, on the brightest day that has occurred since I received the apparatus, the 2nd of October 1829, therm. 52°, the diurnal variation at noon was 26′ 20″ W. On the two gloomiest days that have occurred, the 3rd and 4th of December, 1829, therm. about 42°, the diurnal variation at noon was 3′ 20″ and 3′ 40″ respectively. Again, some observations in the month of January last, on bright days, when yet the earth derived little or no heat from the sun, owing to a close covering of snow and a hard frosty wind,—and others, on bright days likewise, when the earth, much cleared of snow, imbibed much heat,—would lead me to believe, that the direct radiating heat of the sun has more effect than the light in causing the increased variation, when the state of the earth's surface is such as to imbibe that heat.

VII. *Remarks on several icebergs which have been met with in unusually low latitudes in the southern hemisphere.* By Captain James Horsburgh, *Hydrographer to the East India Company, F.R.S.*

Read Feb. 4, 1830.

An apology might probably be necessary, in offering the following remarks to the notice of the Royal Society, if they did not pertain to a phenomenon of a novel nature, which may be interesting to those members of the Society who are anxious for the elucidation of cosmographical science.

It appears that icebergs, until lately, have seldom been seen by navigators in their passage near the Cape of Good Hope and the coast of South Africa; for the journals of the ships belonging to the East India Company, during the whole of the last century, do not specify that any icebergs had been seen in the route of their navigation in the southern hemisphere, although several of these ships proceeded into the parallels of latitude 40°, 41°, and 42° south.

On April the 7th, 1828, the Harmonie, French ship from Calcutta, bound homeward, in latitude 35° 50′ south, longitude 18° 5′ east of Greenwich, saw several icebergs, some of them appearing to be 100 feet elevated above the sea, and passed between two of them, upon which the sea broke violently. When amongst these icebergs, the Harmonie fell in with the Spanish ship Constancia from Manilla, bound to Cadiz, the pilot of which describes them as follows:

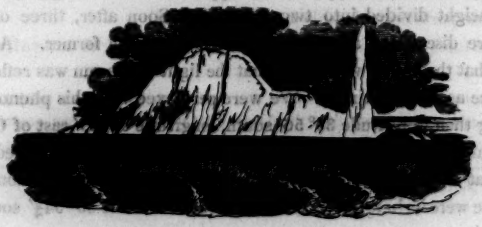
April 7th, 1828, at 10¾ A.M. saw a small island, appearing like a white cloud, and soon afterward some shadowy lines were observed in it, as is usual in land: upon a nearer approach, it appeared to be a large island of considerable height divided into two summits. Soon after, three other small islands were discovered at a short distance from the former. At 11½ A.M., perceived that they were white, and that the light of the sun was reflected from their surface as from a mirror. We were perplexed with this phenomenon till noon, being then in latitude 35° 56′ south, longitude 16° 59′ east of Greenwich, by chronometer, corresponding with lunar observations. Sounded, but got no bottom at 135 fathoms; and the sea continuing of a green colour, we concluded these were icebergs, which had drifted to latitude 35° 54¼′ south, longi-

tude 17° 59′ east. Steered W.S.W. till 2 P.M. and spoke the French vessel L'Harmonie from Calcutta. At 3½ P.M. discovered two more icebergs, which we passed at 4½ P.M., the most southerly of these presenting a square of 25 or 30 toises of elevation, but without an apex like the other near it. At the distance of three miles to the north of these, another iceberg of large size appeared.

On the 28th of April, 1828, the brig Eliza from Antwerp, bound to Batavia, fell in with five icebergs in latitude 37° 31′ south, longitude 18° 17′ east of Greenwich, having the appearance of church steeples, and apparently from 250 to 300 feet high, which were passed within the distance of a quarter of a mile; and the sea broke so violently against these enormous masses of ice, that at first they were thought to be fixed on some unknown shoal, until on sounding, no bottom could be obtained.

These icebergs, seen by the Eliza three weeks after the former were discovered by the ships Harmonie and Constancia, although nearly on the same meridian, were 32 leagues to the south of those first seen, and therefore must have been different masses following nearly in the track of the former, and carried along from a high southern latitude by the current and waves towards the coast of South Africa.

The East India Company's ship Farquharson, April 20th, 1829, in latitude 39° 13′ south, longitude 48°46′ east, fell in with a large iceberg, and when near it the measured altitude gave 150 feet for its height above the surface of the sea, and it appeared to be about two miles in circumference. If the height measured from an altitude of this iceberg be correct, the whole height from its base to the apex must have been 1000 feet or upward, by allowing for the difference of gravity between ice and water, compared with the form of the iceberg above the sea. When near it, the annexed view was taken.



Antecedent to these icebergs, discovered in April 1828, and April 1829, none appear to have ever been seen to the northward of the 42nd or 43rd degree of south latitude, in the Southern Ocean; but His Majesty's store-ship Guardian struck upon one in latitude 44° 10′ south, longitude 44° 25′ east, on the 24th December 1789.

In the Encyclopedia by the late Dr. Rees, it is stated that "floating ice has occasionally been found in both hemispheres as far as 40° from the poles, and sometimes, as has been said, even in latitudes 41° and 42°." But it is now ascertained that icebergs are carried to a greater distance from the poles before they are dissolved.

As the icebergs in the southern hemisphere have been found further from the pole in April than at any other time, it might be expected that in the corresponding month of October they would in the northern hemisphere be found at the greatest distance from the Arctic pole; it however appears that in the same month of April or May*, icebergs have been seen at the greatest distance from the latitude of their formation, in the northern as well as in the southern hemisphere; in accordance with which the following examples may be stated:

April 14th, 1817, the Minerva from New York, bound to Liverpool, fell in with four large icebergs in latitude 42° 47′ north, longitude 47° west.

April 3rd, 1823, the Mountstone sailed from Plymouth for St. Johns Newfoundland, and on the 7th of May struck against a mass of ice during a thick fog, and shortly afterward filled with water:—the latitude not given.

May 14th, 1814, the fleet bound to Quebec, in latitude 44° 18′ north, longitude 50° 50′ west, fell in with upwards of twenty large icebergs, some of which were 80 feet above water; and in the afternoon of the same day the convoy passed a field of ice about 20 miles in extent, and about 30 feet elevated above the sea, some parts considerably higher.

From the foregoing observations, the following remarks naturally arise:

* There are, however, some exceptions to this remark; as icebergs (it is said) have been seen in July or August not far distant from the Azores; but perhaps this seldom occurs, for in these months southerly winds prevail, with a warm atmosphere, and frequently a current setting to the northward: all, together, forming a barrier of resistance to the advancement of icebergs to a low altitude in the North Atlantic Ocean.

First,—That in April, or early in May, both in the northern and southern hemisphere, icebergs, or large masses of ice, have been found in lower latitudes than at other times, which appears to be anomalous, as a difference of six months in time might be expected between the nearest approach to the equator of the northern and southern icebergs.

Secondly,—That the existence of a large tract of land near the Antarctic circle seems to be necessary for the origin and accretion of the southern icebergs, and probably situated somewhere between the meridian of London and longitude 20° east; from whence these icebergs have been carried in a N. and N.N.easterly direction by the united forces of current, winds, and waves, prevailing from S.S.W. and S.W.; for Sandwich Land, in latitude about 60° south, longitude 27° west, seems to be too far west from the prevailing line of direction of the currents, winds, and waves, to be the place of formation of the icebergs found near the Cape of Good Hope, and of that seen by the Farquharson. Bouvet's Island, and Thompson's Island, in latitude about 54° south, longitude 5½° east, are not of magnitude sufficient to have been the basis of these icebergs; and Kerguelen's Island, in latitude 49° south, longitude 70° east, is too far to the eastward to have been their original base.

Thirdly,—That from the unprecedented appearance of icebergs in the vicinity of the south coast of Africa in April 1828, and in April 1829 further to the eastward, some unknown cause probably produced a disruption of these icebergs from the place of their formation, such as an earthquake, or volcanic forces, which seldom or never had before happened in those regions; but more particularly during the last century, a period when icebergs have not been seen in the Southern Ocean near the coast of South Africa.

If, however, these icebergs were dislocated from the land by some unknown cause, which seldom or never had before happened, the anomaly adverted to above, of the northern and southern icebergs appearing in the lowest latitudes in April or May, would in such case only apply to these icebergs having been carried from the polar regions by similar currents in the same months, instead of a difference of six months, in reference to the set of the North Atlantic current and that of the Southern Ocean, as might have been expected.

VIII. *On the progressive improvements made in the efficiency of steam engines in Cornwall, with investigations of the methods best adapted for imparting great angular velocities.* *By* DAVIES GILBERT, *Esq. President.*

Read March 4, 1830.

IN the year 1827, some observations I had made on steam engines were honoured by a place in the Philosophical Transactions. I am therefore induced to lay before the Society further particulars illustrative of the progress by which that most important machine has reached its actual high state of improvement. On a subject of less magnitude I should not have presented to the Society a mere collection of matter in detail, unconnected by any general arrangement of the facts: but every thing appears to me of great interest that bears on the history of an invention that has continually advanced towards perfection by the aid of chemical, mechanical, and mathematical sciences; an invention that has already altered and improved the condition of mankind; and seems destined to produce consequences the most beneficial to civilized society, by extending the dominion of intellect over muscular power and brute force. I am moreover desirous of preserving information derived from documents which have never yet passed out of private hands, and are consequently liable to be lost or destroyed.

For all practical purposes the steam engine must be considered as originating with Mr. NEWCOMEN; the introduction of a moveable diaphragm between the active power and the vacuum or less elastic medium, being essential to the very principle of the machine as a moving power.

Mr. NEWCOMEN's engines were brought into Cornwall very early in the last century, where they immediately superseded the laborious method of drawing water by human exertions, applied through the simple medium of a chain pump, similar in construction to those at present used on board large ships. So inartificial, indeed, was the machinery in mines at this comparatively recent

period, that I well remember an individual who used to boast of his having assisted in constructing the first Whim, or contrivance for applying the strength of horses to the lifting of weights, that was seen in the peninsula west of Hayle and of St. Michael's Mount.

The only material improvement ever made to Mr. NEWCOMEN's engine, had, I apprehend, been effected previously to its introduction into Cornwall, which consisted in the automatic opening and shutting of the valves. It consequently remained in nearly the same state up to the time when a new æra commenced from the appearance of Messrs. BOULTON and WATT.

The use of Mr. WATT's engine, secured to him by a patent in 1769, and extended by Parliament to the year 1800, was offered on terms reasonable in themselves, and fairly growing out of the subject. These were, payments of one third part of all the savings in fuel, to be estimated by a comparison of the new engines with those on the former construction executing equal quantities of work.

Experiments on a large scale were accordingly instituted in the summer of 1778 on two atmospheric engines, then supposed to be the most perfect of any at work in Cornwall; conducted by Messrs. BOULTON and WATT themselves on one part, and by the engineers and managers of mines on the other, assisted by some of the most respectable gentlemen of the county, who were interested either as proprietors of the land, or as adventurers in the mines.

The following is a copy of their Report:

"Poldice Mines, October 30, 1778.

"We the under subscribers having carefully examined the books of the mine touching the consumption of coals of the two eastern engines during the months of August and September of this present year, and attended to the working of the engines during those months, do hereby certify, that the said two eastern engines did consume in 61 days of those two months 220 weys of coal, each wey being 64 Winchester coal bushels, which amount to 14'080 bushels in 61 days.

"We do also certify, that the said two engines together do work pumps in four lifts which are 17 inches diameter in the working barrels, and the whole depth from whence these pumps drew water to the adit is 58 fathoms.

"We further certify, that the said engines did during those months of August

and September last, work the said pumps at the rate of 6 strokes of 5½ feet long each in every minute, which amounts to 8640 strokes per 24 hours.

"We have also made an accurate calculation, by which it appears that when the new fire-engine to be erected by Messrs. Boulton and Watt is completed, and actually works a pump of the same depth of 58 fathoms and 17 inches in diameter at the rate of 6 strokes of 5½ feet long each in a minute, and consequently making 8640 strokes per 24 hours, it will draw a quantity of water equal to that now drawn by both the present engines, and consequently whatever smaller quantity of coals it uses than 14'080 bushels for 61 days when going at the rate of 6 strokes per minute, will be the real savings in fuel occasioned by the said new engine at that rate of going.

(Signed) "James Watt.
"Matthew Boulton.
"H. Hawkins Tremayne.
"Richard Williams.
"John Williams.
"Thomas Brown."

A pound avoirdupoise lifted through one foot had not at that period been established as the dynamic unit.

The product of pounds raised, and of the number of feet through which they are lifted in a given time, divided by the number of bushels of coal (supposed to weigh 84 pounds) burnt in the same interval, give what is now termed the duty of the engine, and afford a perfect criterion of its comparative merit.

The most convenient method of forming this estimate is by multiplying together, the diameter in inches squared, of the lifting box, or of the plunger piston, the height of the lifts in fathoms, and 2.04 (log. 3.3095101) the weight in pounds avoirdupoise of a cylinder of water one inch in diameter and six feet long, the product gives the weight.

This, multiplied by the length of the stroke in feet, and by the number of strokes in a given time, and finally divided by the number of bushels of coal consumed, will give the duty.

In the certified case,

Inches in the diameter of the lifting box 17, sq. 289 . . .	Log. 2.4608978
Length of the column in fathoms 58	1.7634280
Constant multiplier 2.04	0.3095101
Weight in pounds 34185	4.5338359
Length of lifts 6 feet × 5½ the number in a minute = 33, 33 multiplied by 1440 = 47520, and this number × by 61 = 2'898720	6.4622063
Pounds into feet during the whole period of 61 days 99092'800000	10.9960422
Bushels of coal consumed in the 61 days 14.080	4.1486027
The duty 7'037800	6.8474395

If the quantity of water lifted at each stroke is required in imperial gallons; square as before the diameter of the lifting box or plunger in inches, multiply by the length of the stroke in feet, and by 0.034 (the fractional part of an imperial gallon in a cylinder one inch in diameter and a foot long—the logarithm 8.5313588): the product will be the gallons raised.

Diameter in inches squared as before	Log. 2.4608978
6 feet	0.7781513
Constant multiplier 0.034	8.5313588
Number of gallons 58.94	1.7704079
5½ strokes in a minute, × minutes in 61 days 87'840 = 483.120	5.6840550
Imperial gallons in 61 days = 28'475000	7.4544629

Each separate engine on the new construction underwent a constant comparison, during the whole time of its continuing in action, with the duty of the two standard atmospheric engines as reported by the committee.

The diameter of the various lifting boxes or plungers, the length of the lifts or columns of water, and the lengths of the strokes, were matters of common notoriety; and the number of times moved by each engine in a given period was ascertained by a contrivance denominated a Counter, placed on the great beam; this apparatus involves a series of wheels and pinions set in motion by

a weight rolling in each direction, and acting through the medium of an escapement similar to that of a clock.

In the year 1793, that is fifteen years afterwards, an account was taken of the work performed by seventeen engines on Mr. WATT's construction, then working in Cornwall. This statement is not verified in the authentic manner of the former; but there do not appear to be any reasons for doubting of its correctness. The average duty was 19'569000 pounds of water raised one foot high, by the consumption of one bushel of coal; exceeding the standard experiment on the two atmospheric engines in the proportion of 2.78 to 1, or nearly as 2¾ to 1. So that work requiring by the atmospheric engines 278 bushels of coal, would be performed by Mr. WATT's steam engine by 100 bushels, consequently 178 would be saved. One third part of these must have been paid to Messrs. BOULTON and WATT as patentees, leaving a clear gain to the mine of 118 bushels, being more than the quantity consumed.

Some years later, disputes took place as to the real performance of Mr. WATT's engines, and a reference was agreed on between the parties to five individuals, of whom I had the honour to be one; and in May 1798 returns were made by the agents in various mines, of all the particulars respecting twenty-three engines, from which I then deduced their respective duties for the information of the referees.

It will not be necessary to trouble the Society with more of these particulars than the diameter of the cylinder, whether it worked single or condensed, both above and below, and the final result or duty in millions of pounds lifted one foot high by the consumption of one bushel of coals.

	Diameter of Cylinder in inches.	Duty.	Observations.
1	20	10'015000	
2	21	16'385000	
3	45	29'668000	It was believed at the time that some inaccuracy must have occurred in the communications respecting these two engines.
4	36	28'212000	
Double 5	42	18'193000	On the same mine. The length of strokes in all but one, six feet; in that, eight feet. Average duty of the whole, 15'985000.
6	63	15'190000	
Double 7	45	15'180000	
8	45	15'571000	
9	45	15'090000	
10	45	14'384000	
Double 11	42	18'740000	
12	42	15'532000	
13	36	18'465000	
14	—	12'226000	The diameter of the cylinder not returned.
15	30	14'050000	
16	20	12'366000	
Double 17	14½	6'097000	
18	30	13'931000	
19	28	19'739000	
20	36	24'514000	
21	21	13'215000	
22	20	15'034000	
23	48	27'503000	Supposed to be the best engine.
		17'671000	The general Average in 1798.

It may be observed that the average duty was here somewhat less, than it had been found in 1793, confirmatory of an opinion generally entertained that the steam engines had deteriorated from the time of Mr. WATT's quitting his residence near Redruth.

The principles and even the mechanism of Mr. WATT's engines have remained unaltered since their first introduction, unless a change in the precise periods of opening and closing the valves could be considered a variation. But to such an extent has the economy of fuel been carried by the use of steam at a high degree of temperature and consequently of pressure, usually from fifty to sixty inches of mercury above the atmosphere, by extending the expansive action to two thirds or even to three quarters of the whole descent of the piston, by making small fire-places, with sharper drafts, in iron tubes surrounded by the water of the boiler, by more effectually preventing the escape of heat, by enlarging the engines themselves, and perhaps by executing the work with superior accuracy, that in the monthly return of duty performed in Cornwall by the steam engines in December 1829,—the best engine with

a cylinder of 80 inches did 75'628000, exceeding the duty performed in 1795 in the proportion of 3.865 to 1, or as 27 : 7 nearly: and exceeding the standard atmospheric engines of 1778 by 10.75 : 1. But subject as in former times to a great variation between different engines apparently similar in all respects; the average being about forty-one million and a half.

Greatly as we are indebted to Mr. WATT for his improvement of the steam engine used in exhausting water from mines, our obligations to him are still greater for originating and carrying almost to a state of perfection, the application of steam as a moving power to machinery, in all the complicated and varied uses of mechanical inventions in this country.

In effecting this most important object, the double engine was first brought into use, the extremely ingenious contrivance for producing parallel motion was invented, and the principle of centrifugal force enabled an apparatus called a Governor to regulate a supply of steam inversely proportionate to the velocity which might at any instant be acquired; and the use of fly-wheels, perfectly understood in theory, became subservient to the regularity of motion, and to the gigantic efforts of our most ponderous machinery. We owe further to Mr. WATT the introduction, at least into general use, of what is termed bevelled geer.

When wheels acting on each other by means of teeth have their axes parallel, the teeth however curved on themselves, must obviously be parallel also on their line of contact. But when the axes are inclined, the line of contact between the teeth should then be directed to the point where the two axes would intersect, thus assimilating their action to that of a cone revolving round a centre on a circular plane.

Having on various occasions had my attention drawn to the consideration of machines forced into rapid action by great powers moving at a comparatively slow rate, I have been induced to make endeavours for ascertaining the amount of friction in several instances.

The mode adopted has been to impart equal velocities to the machine performing the work for which it was destined, and in a state disincumbered from all extraneous impediments, and then to compare the forces applied under these different circumstances. The media of communication being in all cases axes with wheels, impelled by teeth, or cogs.

In some instances the friction was found to equal two-thirds of the whole

resistance, in others a half, and in some few instances one-third: evidently becoming less as the wheels were increased in size; and always greatly diminished by the introduction of an additional axis for multiplying the angular velocity. In similar machines recently constructed on the best principles, friction is said not to exceed a tenth.

With the hope of reducing the amount of this great impediment to the useful application of motive force, I have been led to consider what would be the most proper form of teeth or cogs, and by how many intermediate steps a given increase of angular velocity might be most advantageously effected.

It is quite clear, with respect to cogs or teeth, that to give them theoretical perfection, they should be so formed as to communicate an equable velocity from the driving to the driven wheel; and at the same time to roll upon themselves free from any sliding, the cause of friction and of abrasion.

Either of these properties may be separately obtained; but the two are utterly incompatible, except at the limit where teeth disappear and the wheels themselves are in contact.

For producing equable velocities.

The involute from a circle, receives continually an accession of length to its radius of curvature equal to the development of the generative arc. Consequently, if involutes are formed in opposite directions from two circles of magnitudes inversely proportionate to the angular velocities of their respective wheels, the extremities of the two radii of curvature will always remain in contact, forming together a tangent to the evolute circles crossing the line of the centres. The angular velocities must therefore be uniform, while the surfaces will slide on each other, and create a friction proportionate to the difference of length between the radii of curvature.

For avoiding friction between the cogs or teeth.

As logarithmic spirals preserve always a constant angle between the ordinate and the curve, if two similar logarithmic spirals act against each other, they must continue to roll without friction, the ordinates remaining in contact on the line of the centres, but causing angular velocities in the inverse proportion of those ordinates.

It is plain therefore that the two properties are incompatible, since the loci of contact between the curves producing them are in different lines. But since the involutes may be generated from circles indefinitely small, without refer-

ence to the size of the wheels, and the logarithmic spirals may be inclined at any angle: at their ultimate limit the lines of the centres and of the tangents will coincide, producing equable velocities and avoiding friction; but at this point the teeth or cogs disappear, and the wheels revolve with a contact of their peripheries.

As cogs become oblique, their contact produces an increase of pressure with augmented friction on the axes: but I am induced to believe, from the results of such trials as I have been enabled to make, as well as from theory, that an uniform angular motion is the great object to be sought, especially in ponderous machinery, and above all where fly-wheels are applied. The expedient usually resorted to, of making the teeth small and consequently many in number, renders attention to any precise curvature less necessary; but this subdivision is limited by want of strength; and I would venture to recommend the involute form, producing an equability of angular motion.

When angular velocities are to be multiplied in large machines, the effect can scarcely be produced in any other way than by the usual one, of wheels unequal in diameter and consequently in the numbers of their teeth.

The question then to be resolved is this:

By the interposition of how many such pairs of wheels can the ultimate angular velocity be most advantageously produced?

Wishing to obtain a solution accurate to no further degree than what may be sufficient for the guidance of practical experiments, I omit to take any separate account of the friction caused by the teeth of the wheels as distinct from that of their axes, and I shall consider the wheels and axes throughout the train in a constant ratio to each other, although as the actual pressure becomes less, the axes may be reduced in size, and thereby friction diminished.

When two forces act on an axis from the peripheries of two wheels attached to it; one for receiving motion, and the other for conveying it on through the train, the pressure on the axis may be the sum of the two forces, their difference, or any intermediate quantity according to the positions of the points communicating with the preceding and with the subsequent wheels; the pressure is most usually their sum, as convenience generally requires that the power should be received and carried forwards at opposite points.

Let the diameters of the two wheels be as $1 : d$, then will the amount of pressure on the axis be $d + 1$. Assuming the axis and wheels next in succession to be similar in all respects, the pressure on them will be less than on the former in proportion to the increased angular velocity, but the prime mover having the disadvantage of leverage in the same proportion, the retarding effect of friction will be precisely equal in both; whence it is obvious, that the same ratio should continue throughout all the series, or that the multiplication of angular velocity should proceed in a geometrical proportion.

Let then the whole increase of angular velocity be represented by a, and let the number of axes employed be x. Then in the usual mode of applying the wheels the friction on each axis will be $a^{\frac{1}{x}} + 1$. And the friction of the whole of the axes will be $(a^{\frac{1}{x}} + 1)\ x$. To find when this is a maximum, let A = the natural logarithms of a. Then will the fluxion of $(a^{\frac{1}{x}} + 1).x$ be

$$- \mathrm{A} \,.\, a^{\frac{1}{x}}.x^{-1} \dot{x} + a^{\frac{1}{x}} \dot{x} + \dot{x} \text{ when this is put} = 0$$

$$x = \frac{\mathrm{A}}{1 + a^{-\frac{1}{x}}}$$

By approximating it will be found that when

$a = 120$ x or the number of axes $= 3.745$, and $(a^{\frac{1}{x}} + 1).x$ or the friction 17.9 instead of 121 if x were unity and the whole angular velocity communicated at once, about one seventh part.

$a = 100$ $x = 3.6$ friction 15.64 about 10 parts in 64 of the whole, as above.

$a = 40$ $x = 2.88$ friction 13.25, about one third part of 41.

When $a =$ 3.59 the minimum of friction falls on a single axis.

$a =$ 12.85 the minimum of friction is on two axes.

$a =$ 46.3 the minimum of friction is on three axes.

$a =$ 166.4 the minimum falls on four axes.

In practice it is obvious that x must represent a whole number.

Let $a = 64$ x may be either two or three.

If $x = 2$ the friction will be 18.

3 the friction will be 15.

If the wheels be so arranged that the resultant of the two forces on the axes equal the larger in magnitude, x becomes = to A, and the amount of friction $a^{\frac{1}{A}} \times A$ but $a^{\frac{1}{A}} =$ the radix of the natural system of logarithms; consequently the friction in this case will be $e \times A$.

If the points of the two wheels receiving and communicating motion are placed in the same right line parallel to the axes, the general expression becomes $x = \frac{A}{1-a}^{-\frac{1}{x}}$ evidently giving to x an infinite magnitude: but this can indicate no more in practice than the advantage of so arranging the communications of motion when it can possibly be done.

As fly-wheels are of essential use in preserving uniform velocities, or for accumulating power in almost all rotatory movements produced from those that alternate, and as the power of the centrifugal force has sometimes exceeded the cohesion even of iron, I shall conclude this paper with an adaptation of a well-known theorem to common use.

Let r = the radius of a wheel expressed in feet.

v = the velocity of the rim where all the weight is supposed to be accumulated, expressed in feet in a second.

s = the space in feet through which a body descends in one second 16.0899; log. 1.2065541.

F = the centrifugal force.

$$\text{Then } F = \frac{v^2}{2r.s}$$

Let n = the number of revolutions in a second.

c = the periphery of the circle to diameter unity = 3.14159.

log. 0.4971499.

$$\text{Then } F = r.n^2 \times 1.2268 \text{ (log. 0.0887757).}$$

Consequently for an approximate value,

The centrifugal force = the radius × number of revolutions in a second squared × 1.2.

If N = the number of revolutions in a minute, the product found as above must be divided by $60 \times 60 = 3600$; consequently $F = r N^2 \times 0.00034078$ (log. 6.5324732.)

For an approximate value,

The centrifugal force = the radius × number of revolutions in a minute squared × 34 and cutting off five places of figures.

IX. *On the laws of the polarization of light by refraction.* *By* DAVID BREWSTER, *LL.D. F.R.S. L. & E.*

Read Feb. 25, 1830.

IN the autumn of 1813 I announced to the Royal Society the discovery which I had then made of the polarization of light by refraction*; and in the November following I communicated an extensive series of experiments which established the general law of the phænomena. During the sixteen years which have since elapsed, the subject does not seem to have made any progress. From experiments indeed stated to have been performed at all angles of incidence with plates of glass, M. ARAGO announced that the quantity of light which the plate polarized by reflexion at any given angle was equal to the quantity polarized by transmission; but this result, founded upon incorrect observation, led to false views, and thus contributed to stop the progress of this branch of optics.

I had shown in 1813, from incontrovertible experiments, that the action of each refracting surface in polarizing light, produced a physical change on the refracted pencil, and brought it into a state approaching more and more to that of complete polarization. But this result, which will be presently demonstrated, was opposed as hypothetical by Dr. YOUNG and the French philosophers; and Mr. HERSCHEL has more recently given it as his decision, that of the two contending opinions, that which was first asserted by MALUS, and subsequently maintained by BIOT, ARAGO, and FRESNEL, is the most probable,—namely, that the unpolarized part of the pencil, in place of having suffered any physical change, retains the condition of common light.

I shall now proceed to apply to this subject the same principles which I

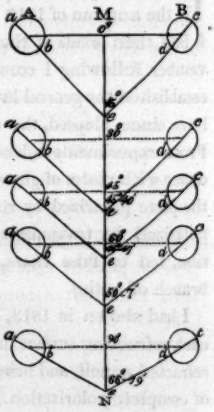
* In this discovery I was anticipated by MALUS.

have already applied to the polarization of light by reflexion, and to establish on the basis of actual experiment the true laws of the phenomena.

The first step in this inquiry is to ascertain the law according to which the polarizing force of the refracting surface changes the position of the planes of polarized light,—a subject which, in as far as I know, has not occupied the attention of any other person.

If we take a plate of glass deviating so slightly from parallelism as to throw off from the principal image the images formed by reflexion from its inner surfaces, we shall be able to see, even at great obliquities, the transmitted light free from all admixture of reflected light. Let this plate be placed upon a divided circle, so that we can observe through it two luminous discs of polarized light A, B (Fig. 1.) formed by double refraction, and having their planes of polarization inclined + 45° and − 45° to the plane of refraction. At an angle of incidence of 0°, when the light passes perpendicularly, the inclination of the planes of polarization will suffer no change; but at an incidence of 30° they will be turned round 40′; so that their inclination to M N or the angle *aec* will be 45° 40′. At 45° their inclination will be 46° 47′. At 60° it will be 50° 7′; and it will increase gradually to 90°, where it becomes 66° 19′. Hence the maximum change produced by a single plate of glass upon the planes of polarization is 66° 19′ − 45° = 21° 19′, an effect exactly equal to what is produced by reflexion at angles of 39 or 70°.

Fig. 1.



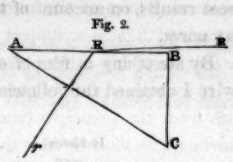
It is remarkable, however, that this change is made in the opposite direction, the planes of polarization now approaching to coincidence in a plane at right angles to that of reflexion. This difference is exactly what might have been expected from the opposite character of the resulting polarization, the poles of the particles of light which were formerly repelled by the force of reflexion, being now attracted by the refracting force.

In this experiment the action of the two surfaces is developed in succession, so that we cannot deduce from the maximum rotation of 21° 19′, the real

action of the first, or of a single surface, which must be obviously more than half of the action of the two surfaces, because the planes of polarization have been widened before they undergo the action of the second surface.

In order to obtain the rotation due to a single surface, I took a prism of glass A B C (Fig. 2.) having such an angle B A C, that a ray R R, incident as obliquely as possible, should emerge in a direction R *r* perpendicular to the surface A C. I took care that this prism was well annealed, and I caused the refraction to be performed as near as possible to the vertex A, where the glass was thinnest and consequently most free from the influence of any polarizing structure. In this way I obtained the following measures.

Fig. 2.



GLASS.

Angles of Incidence.	Inclination of Planes *ab*, *cd*, (Fig. 1.) to the Plane of Reflexion.	Rotation.
87° 38′	54° 15′	9° 15′
54 50	47 25	2 25
32 20	45 22	0 22

I next made the following experiments with two kinds of glass,—the one a piece of parallel plate glass, and the other a piece of very thin crown. The latter had the advantage of separating the reflected from the transmitted light.

	PLATE GLASS.		CROWN GLASS.	
Incidence.	Inclination.	Rotation.	Inclination.	Rotation.
0°	45° 0′	0° 0′	45° 0′	0° 0′
40	47 28	2 28	47 18	2 18
55	49 35	4 35	49 19	4 19
67	52 53	7 53	52 16	7 16
80	58 53	13 53	58 42	13 42
86½	61 16	16 16	61 0	16 0

I was now desirous of ascertaining the influence of refractive power, although I had already determined in 1813, that a greater quantity of light was polarized, at the same angle of incidence, by plates of a high than by plates of a

low refractive power. I experienced great difficulty in this part of the inquiry, from the necessity of having plates without any crystalline structure. I tried gold leaf in a variety of ways; but I found it almost impossible to obtain correct results, on account of the light which was transmitted unchanged through its pores.

By stretching a film of soapy water across a rectangular frame of copper wire I obtained the following measure.

WATER.

Incidence.	Inclination.	Rotation.
85°	54° 17′	9° 17′

I next tried a thin plate of metalline glass of a very high refractive power.

METALLINE GLASS.

Incidence.	Inclination.	Rotation.
0°	45° 0′	0° 0′
20	45 42	0 42
30	46 50	1 50
40	48 0	3 0
55	51 12	6 12
80	62 32	17 32

From a comparison of these results it is manifest that the rotation increases with the refractive power.

In examining the effects produced at different angles of incidence, it becomes obvious that the rotation varies with the deviation of the refracted ray; that is, with $i - i'$ the difference of the angles of incidence and refraction. Hence from a consideration of the circumstances of the phænomena I have been led to express the inclination φ of the planes of polarization to the plane of refraction by the formula,

$$\text{Cot}\,\varphi = \cos\,(i - i'),$$

the rotation being $= \varphi - 45°$.

This formula obviously gives a minimum at 0°, and a maximum at 90°; and at intermediate points it represents the experiments so accurately, that when the rhomb of calcareous spar is set to the calculated angle of inclination, the extraordinary image is completely invisible,—a striking test of the correctness of the principle on which it is founded.

The above expression is of course suited only to the case where the inclina-

tion x of the planes of polarization *ab*, *cd*, (Fig. 1,) is 45°; but when this is not the case, the general expression is

$$\text{Cot } \varphi = \cot x \cos (i - i').$$

When the light passes through a second surface, as in a single plate of glass, the value of x for the second surface is evidently the value of φ after the 1st refraction, or in general, calling θ the inclination after any number n of refractions, and φ the inclination after one refraction,

$$\text{Cot } \theta = (\cot \varphi)^n$$

When θ is given by observation we have

$$\text{Cot } \varphi = \sqrt[n]{\cot \theta}.$$

The general formula for any inclination x and any number n of refractions is

$$\text{Cot } \theta = \left(\cot x \cos (i - i')\right)^n, \text{ and}$$

$$\text{Cot } \varphi = \sqrt[n]{\cot x \cos (i - i')}.$$

And when $x = 45$ and $\cot x = 1$ as in common light,

$$\text{Cot } \theta = \left(\cos (i - i')\right)^n.$$

$$\text{Cot } \varphi = \sqrt[n]{\cos (i - i')}.$$

As the term $\left(\cos (i - i')\right)^n$ can never become equal to 0, the planes of polarization can never be brought into a state of coincidence in a plane perpendicular to that of reflexion, either at the polarizing angle, or at any other angle.

In order to compare the formula with experiment, I took a plate of well annealed glass, which at all incidences separates the reflected from the transmitted rays, and in which m was nearly 1.510, and I obtained the following results.

Angles of Incidence.	Angles of Refraction.	Rotation observed.	Inclination observed.	Inclination calculated.	Difference.
0°	0° 0′	0° 0′	45° 0′	45° 0′	
10	6 36½	0 13	45 13	45 6	+0° 7′
20	13 5	0 27	45 27	45 25	+0 2
25	16 15	0 32	45 32	45 40	−0 8
30	19 20	0 40	45 40	46 0	−0 20
35	22 19	1 12	46 12	46 25	−0 13
40	25 10	1 30	46 30	46 56	−0 26
45	27 55	1 42	46 47	47 34	+0 47
50	30 29	2 48	47 42	48 24	−0 42
55	33 52	3 54	48 54	48 59	−0 5
60	35 0	5 7	50 7	50 36	−0 29
65	36 53	6 48	51 48	52 7	−0 19
70	38 29	8 7	53 7	53 59	−0 52
75	39 45	9 55	54 55	56 18	−1 23
80	40 42	12 10	57 10	59 5	−1 55
85	41 17	15 45	60 45	62 24	−1 39
86	41 21	16 39	61 39	63 9	−1 30
90	41 28			66 19	

The last column but one of the Table was calculated by the formula,

$$\text{Cot } \theta = \left(\cos\ (i - i')\right)^2$$

n being in this case 2. The conformity of the observed with the calculated results is sufficiently great, the average difference being only 41′. The errors however being almost all negative, I suspected that there was an error of adjustment in the apparatus; and upon repeating the experiment at 80°, the point of maximum error, I found that the inclination was fully 58° 40′, giving a difference only of 25′ in place of 1° 55′. I did not think it necessary to repeat all the observations; but I found, by placing the analysing rhomb at the calculated inclinations, that the extraordinary image invariably disappeared, the best of all proofs of the correctness of the formula.

In these experiments $x = 45°$ and $\cot x = 1$; but in order to try the formula when x varied from 0° to 90°, I took the case where the angle of incidence was 80° and $\varphi = 58°\ 40'$ when $x = 45°$. The following were the results.

Values of x.	Inclination observed.	Inclination calculated.	Difference.
0°	0° 0′	0° 0′	0° 0′
$2\frac{1}{2}$	7 10	7 20	−0 10
5	9 40	8 19	+1 21
10	17 10	16 25	+0 45
15	24 42	24 6	+0 36
20	32 30	31 19	+1 11
25	39 15	37 54	+1 21
30	44 10	43 57	+0 13
35	49 38	49 28	+0 10
40	54 36	54 31	+0 5
45	58 40	59 5	−0 25
50	63 10	63 19	−0 9
55	66 58	67 15	−0 17
60	70 18	70 56	−0 38
65	74 8	74 24	−0 16
70	76 56	77 42	−0 46
75	79 20	80 53	−1 33
80	83 23	83 58	−0 35
85	86 23	86 0	+0 23
90	90 0	90 0	0 0

The last column but one was calculated by the formula $\cot \theta = \cot x . (\cot 58° 40')^2$. The differences on an average amount only to 36′.

In determining the quantity of polarized light in the refracted pencil, we must follow the method already explained for the reflected ray, *mutatis mutandis*. The principal section of the analysing rhomb being now supposed to be placed in a plane perpendicular to the plane of reflexion, the quantity of light Q' polarized in that plane, will be

$$Q' = 1 - 2\cos^2\varphi,$$

the quantity of transmitted light being unity. But

$$\operatorname{Cot}\varphi = \cot x \cos(i - i'),$$

and as $\cot\varphi = \frac{\cos^2\varphi}{\sin^2\varphi}$ and $\sin^2\varphi + \cos^2\varphi = 1$, we have the quotient and the

sum of $\sin^2 \varphi$ and $\cos^2 \varphi$ to find them. Hence

$$\mathrm{Cos}^2 \varphi = \frac{(\cot x \cos (i - i'))^2}{1 + (\cot x \cos (i - i'))^2}$$

and by substituting this for $\cos^2 \varphi$ in the former equation, it becomes

$$Q' = 1 - 2 \frac{(\cot x \cos (i - i'))^2}{1 + (\cot x \cos (i - i'))^2}$$

Now since by FRESNEL's formula the quantity of reflected light is

$$R = \tfrac{1}{2} \left(\frac{\sin^2 (i - i')}{\sin^2 (i + i')} + \frac{\tan^2 (i - i')}{\tan^2 (i + i')} \right)$$

the quantity of transmitted light T will be

$$T = 1 - \tfrac{1}{2} \left(\frac{\sin^2 (i - i')}{\sin^2 (i + i')} + \frac{\tan^2 (i - i')}{\tan^2 (i + i')} \right)$$

Hence

$$Q' = \left(1 - \tfrac{1}{2} \left(\frac{\sin^2 (i - i')}{\sin^2 (i + i')} + \frac{\tan^2 (i - i')}{\tan^2 (i + i')} \right) \right) \left(1 - 2 \frac{(\cos (i - i'))^2}{1 + (\cos (i - i'))^2} \right)$$

This formula is applicable to common light in which $\cot x = 1$ disappears from the equation; but on the same principles which we have explained in a preceding paper, it becomes for partially polarized rays and for polarized light,

$$Q' = \left(1 - \tfrac{1}{2} \left(\frac{\sin^2 (i - i')}{\sin^2 (i + i')} \cos^2 x + \frac{\tan^2 (i - i')}{\tan^2 (i + i')} \sin^2 x \right) \right) \left(1 - 2 \frac{(\cot x \cos (i - i'))^2}{1 + (\cot x \cos (i - i'))^2} \right)$$

In all these cases the formula expresses the quantity of light really or apparently polarized in the plane of refraction.

As the planes of polarization of a pencil polarized + 45° and − 45° cannot be brought into a state of coincidence by refraction, the quantity of light polarized by refraction can never be mathematically equal to the whole of the transmitted pencil, however numerous be the refractions which it undergoes; or, what is the same thing, refraction cannot produce rays truly polarized, that is, with their planes of polarization parallel.

The preceding analysis of the changes produced on common light, considered as represented by two oppositely polarized pencils, furnishes us with the same conclusions respecting the partial polarization of light by refraction, which we deduced in a preceding paper respecting the partial polarization of light by

reflexion. Each refracting surface produces a change in the position of the planes of polarization, and consequently a physical change upon the transmitted pencil by which it has approached to the state of complete polarization.

This position I shall illustrate by applying the formula to the experiments which I have published in the Philosophical Transactions for 1814.

According to the first of these experiments, the light of a wax candle at the distance of ten or twelve feet is wholly polarized by eight plates, or sixteen surfaces of parallel plate glass at an angle of 78° 52′. Now I have ascertained that a pencil of light of this intensity, will disappear from the extraordinary image, or appear to be completely polarized, provided its planes of polarization do not form an angle of less than $88\frac{3}{4}°$ with the plane of refraction for a moderate number of plates, or $88\frac{1}{2}°$ for a considerable number of plates, the difference arising from the great diminution of the light in passing through the substance of the glass. In the present case the formula gives

$$\text{Cot } \theta = \big(\cos (i - i')\big)^{16} \text{ and } \theta = 88° 50';$$

so that the light should appear to be completely polarized, as it was found to be.

At an angle of 61° 0′ the pencil was polarized by 24 plates or 48 surfaces. Here

$$\text{Cot } \theta = \big(\cos (i - i')\big)^{48} = 89° 36'.$$

At an angle of 43° 34′ the light was polarized by 47 plates or 94 surfaces. Here

$$\text{Cot } \theta = \big(\cos (i - i')\big)^{94} \text{ and } \theta = 88° 27'.$$

It is needless to carry this comparison any further; but it may be interesting to ascertain by the formula the smallest number of refractions which will produce complete polarization. In this case the angle of incidence must be 90°.

Hence $\varphi = 56° 29'$ and $\big(\cos (i - i')\big)^{9}$ gives 88° 36′, and $\big(\cos (i - i)\big)^{10}$ 89° 4′; that is, the polarization will be nearly complete by the most oblique transmission through $4\frac{1}{2}$ plates or 9 surfaces, and will be perfectly complete through 5 plates or 10 surfaces.

Having thus obtained formulæ for the quantity of light polarized by refrac-

tion and reflexion, it becomes a point of great importance to compare the results which they furnish. Calling R the reflected light, these formulæ become

$$Q = R\left(1 - 2\frac{\left(\frac{\cos(i+i')}{\cos(i-i')}\right)^2}{1+\left(\frac{\cos(i+i')}{\cos(i-i')}\right)^2}\right) \text{ and}$$

$$Q' = 1 - R\left(1 - 2\frac{(\cos(i-i'))^2}{\phi + (\cos(i-i'))^2}\right).$$

But these two quantities are exactly equal, and hence we obtain the important general law, that,—At the first surface of all bodies, and at all angles of incidence, the quantity of light polarized by refraction is equal to the quantity polarized by reflection. I have said 'of all bodies', because the law is equally applicable to the surfaces of crystallized and metallic bodies, though the action of their first surface is masked or modified by other causes.

It is obvious from the formula that there must be some angle of incidence where $R = 1 - R$, that is, where the reflected is equal to the transmitted light. When this takes place, we have $\sin^2 \varphi = \cos^2 \varphi'$, that is,

The reflected is equal to the transmitted light, when the inclination of the planes of polarization of the reflected pencil to the plane of reflection, is the complement of the inclination of the planes of polarization of the refracted pencil to the same plane;—or if we refer the inclination of the planes to the two rectangular planes into which the planes of polarization are brought,—The reflected will be equal to the transmitted light when the inclination of the planes of polarization of the reflected pencil to the plane of reflection, is equal to the inclination of the plane of polarization of the refracted pencil to a plane perpendicular to the plane of reflection.

In order to show the connection between the phænomena of the reflected and those of the transmitted light, I have given the following Table, which shows the inclination of the planes of polarization of the reflected and the refracted pencil, and the quantities of light reflected, transmitted, and polarized, at all angles of incidence upon glass, m being equal to 1.525, and the incident light = 1000.

Angles of Incidence, i.	Angles of Refraction, r.	Inclination of Plane of Polarisation of the Reflected Light, φ.	Inclination of Plane of Polarisation of the Refracted Light, φ'.	Quantity of Light Reflected, R.	Quantity of Light transmitted, 1 − R.	Quantity of Light Polarised, Q.
° ′	° ′	° ′	° ′			
0 0	0 0	45 0	45 0	43.23	956.77	0.
2 0	1 18½	44 57	45 0.7	43.26	956.74	0.07
10 0	5 32	43 51	45 3	43.39	956.61	1.73
20 0	12 58	40 13	45 13	43.41	956.59	7.22
25 0	16 5	37 21	45 21	43.64	956.36	11.6
30 0	19 8½	33 40	45 31	44.78	955.22	17.24
35 0	22 6	29 8	45 44	46.33	953.67	24.4
40 0	24 56	23 41	46 0	49.10	950.90	32.2
45 0	27 37½	17 22½	46 20	53.66	946.33	44.0
50 0	30 9	10 18	46 45	61.36	938.64	57.4
56 45	33 15	0 0	47 29	79.5	920.5	79.5
60 0	34 36	5 4½	47 54½	93.31	906.69	91.6
65 0	36 28	12 45	48 42	124.86	875.14	112.7
70 0	38 2	18 32	49 28	162.67	837.33	129.8
75 0	39 18	26 52	50 55	257.56	742.44	152.3
78 0	39 54	30 44	51 48	329.95	670.05	157.6
78 7	39 55	30 53	51 50	333.20	666.80	157.55
79 0	40 4	31 59	52 7	359.27	640.73	157.6
80 40	40 13	33 13	52 27½	391.7	608.3	156.7
82 4	40 35	36 22	53 26½	499.44	500.56	145.4
84 0	40 42	38 2	53 57	560.32	439.68	134.93
85 0	40 47	39 12	54 22	616.28	383.72	123.7
85 50½	40 50½	40 12	54 44	666.44	333.56	111.11
86 0	40 51	40 22 7/9	54 48	676.26	323.74	108.67
87 0	40 54	41 32	55 16	744.11	255.89	89.8
88 0	40 57½	41 23	55 43	819.9	180.1	65.9
89 0	40 58	43 51	56 14	904.81	95.19	36.3
90 0	40 58	45 0	56 29	1000.	0.	0.

It is obvious from a consideration of the principle of the formula for reflected light, that the quantity of polarized light is nothing at 0° because the force which polarizes it is there a minimum. At the maximum polarizing angle, Q is only 79° because the glass is incapable of reflecting more light at that angle, otherwise more would have been polarized. The value of Q then rises to its maximum at 78° 7′, and descends to its minimum at 90°; but the polarizing force has not increased from 56° 45′ to 78° 7′, as the value of φ′ shows. It is only the quantity of reflected light that has increased, which occasions a greater quantity of light to disappear from the extraordinary image of the analysing rhomb.

The case however is different with the refracted light. The value of Q′ has one minimum at 0° and another at 90°, while its maximum is at 78° 7′,

while the force has its minimum at 0° and its maximum at 90°, where its effect is a minimum only because there is no light to polarize. At the incidence of 78° 7′, where the quantities Q, Q' reach their maxima, the reflected light is exactly one half of the transmitted light; $\sin^2 \varphi' = \cos^2 \varphi$ and $\tan \varphi' = \cos \varphi$.

At 85° 50′ 40″, where the transmitted light is one half of the reflected light, the deviation $(i - i') = 45°$, and the quantity of polarized light is one third of the transmitted light, one sixth of the reflected light, and one ninth of the incident light. $\mathrm{Sin}^2 \varphi' : \cos^2 \varphi =$ reflected light : transmitted light, and $\cot \varphi' = \sin (i - i')$.

At 45° we have $(i + i') + (i - i') = 90°$ and $\varphi' = (i - i')$,

$$\mathrm{Tan}\ (i - i') = \frac{\cos (i + i')}{\cos (i - i')}, \text{ and } \tan (i - i')^2 = \frac{(\sin (i - i'))^2}{(\sin (i + i'))^2}$$

At 56° 45′, the polarizing angle, the formula for reflected light becomes $R = \frac{1}{2} \left(\sin^2 (i - i')\right)^2$; but at this angle we have $i' = 90° - i$. Hence we obtain the following simple expression in terms of the angle of incidence, for the quantity of light reflected by all bodies at the polarizing angle.

$$R = \tfrac{1}{2} (\cos 2i)^2.$$

I have already mentioned the experiment of M. Arago with plates of glass, in which he found that "at every possible inclination" the quantity of light polarized by transmission was equal to the quantity polarized by reflexion. This conclusion he extends to single surfaces; but it is remarkable that the law is true of single surfaces in which he did not ascertain it to be true, while it is incorrect with regard to plates in which he believes that he has ascertained it to be true. As the consideration of this point does not strictly belong to the present branch of the inquiry, I shall reserve it for a separate communication, "On the action of the second surfaces of transparent plates upon Light."

Allerly, December 29, 1829.

X. *On the action of the second surfaces of transparent plates upon light.*

By DAVID BREWSTER, *LL.D. F.R.S. Lond. & Edin.*

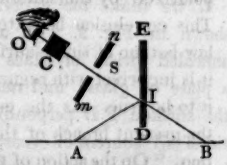
Read February 25, 1830.

IN a paper on the Polarization of Light by Reflexion, published in the Philosophical Transactions for 1815, I showed that the Law of the Tangents was rigorously true for the second surfaces of transparent bodies, provided that the sine of the angle of incidence was less than the reciprocal of the index of refraction. The action of the second surfaces of plates at angles of incidence different from the maximum polarizing angle, was studied by M. ARAGO, who conducted his experiments in the following manner.

"With respect to this phænomenon," says M. ARAGO, "a remarkable result of experiment may here be noticed; that is, that *in every possible inclination* $A = A'$*.

"Let us suppose that a plate of glass E D (Fig. 1.) is placed in the position that the figure represents before a medium A B of a uniform tint; for instance, a sheet of fine white paper. The eye placed at O, will receive simultaneously the ray I O reflected at I, and the ray B I O transmitted at the same point. Place at *m n* an opaque diaphragm blackened, and perforated by a small hole at S. Lastly, let the eye be furnished with a doubly refracting crystal C, which affords two images of the aperture.

Fig. 1.



"If now, by means of a little black screen placed between B and I, we stop the ray B I which would have been transmitted, the crystal properly placed will give an ordinary image $= A + \frac{1}{2}B$, and an extraordinary image

* A is the light polarized by reflexion, and A′ that polarized by refraction.

$= \frac{1}{2}$ B. But if the screen were placed between A and I, and the ray A I were intercepted, we should still have two images of the hole, and their intensities would be $\frac{1}{2}$ B′ and A′ + $\frac{1}{2}$ B′ respectively. Consequently, without any screen, if the whole of the reflected light A I O, and the transmitted B I O are allowed to arrive at the eye, we shall have for the ordinary image A + $\frac{1}{2}$ B + $\frac{1}{2}$ B′, and for the extraordinary image $\frac{1}{2}$ B + A′ + $\frac{1}{2}$ B′.

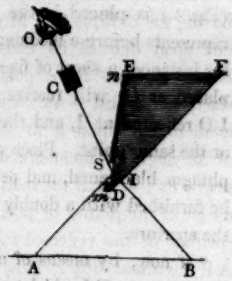
"Now it appears from actually making the experiment, that the two images are perfectly equal, *whatever may be the angle formed by the ray* A I *with the plate of glass* which can only be because A is always equal to A′. Consequently

"The quantity of polarized light contained in the pencil transmitted by a transparent plate is exactly equal, to the quantity of light polarized at right angles, which is found in the pencil reflected by the same plate."

We have no doubt that M. Arago obtained these results, particularly near the polarizing angle, at which limit they are rigorously true; but at all other angles of incidence they are wholly incorrect. When we consider, indeed, the nature of the experiment which has been lauded for its elegance and ingenuity, we shall see reason to pronounce its results as nothing more than coarse estimates, in which the apparent equality of the two images is the effect either of imperfect observation or of some unrecognized compensation.

If we make the experiment in the manner shown in Fig. 2. with a colourless and well annealed prism of glass E F D, in place of a plate of glass; and make the ray B I enter the surface F D perpendicularly at I, we get rid of all sources of error, and we obtain, what is really wanted, the result for a single surface. In this case the experiment is not disturbed by the light reflected from the inner surfaces of the prism, which is all thrown off from the pencil which enters the eye.

Fig. 2.



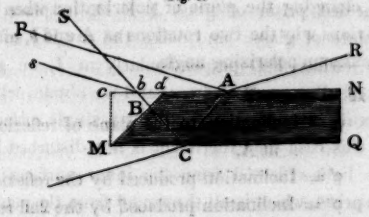
In M. Arago's form of the experiment, part of the ray B I (Fig. 1.) undergoes reflexions within the plate, and there comes along with it to the eye, at O, a portion of light polarized in the plane of reflexion: in like manner the part of the

pencil AI that enters the plate, undergoes partial reflexions, and the part reflected from the first surface carries along with it another portion of light polarized in the plane of reflexion, so that four portions of light polarized in the plane of reflexion reach the eye, while only two portions reach it polarized at right angles to the plane of reflexion, viz. those which are polarized by the refraction of each of the surfaces of the plate. Now the part of the pencil AI which suffers a first reflexion from each of the surfaces of the plate, is, as we shall presently show, defective in polarized light compared with that which has experienced two refractions, so that it requires the above additional quantities to produce a compensation with the transmitted pencil BO. If this is not the true cause of the apparent compensation, that is, if M. Arago took means to exclude the reflected pencils which seem to have produced the compensation, we must then ascribe the equality of the two images to inaccuracy of observation.

But even if we admit that M. Arago's experimental results are correct with regard to plates, it necessarily follows that they cannot be true with regard to surfaces; for it is obvious from the slightest consideration of the subject, that the phænomena of the one can never be interchangeable with those of the other.

In order to demonstrate these views by an analysis of the changes which the intromitted light experiences from the two refractions and the intermediate reflexion of a transparent plate, I took a plate of glass of the shape M N (Fig. 3.) having an oblique face M *d* cut upon one of its ends. A ray of light R A, polarized + 45° and − 45°, was made to fall upon it at A, at an angle of incidence of nearly 83°, so that the inclination of the planes of polarization of the reflected ray A P was about 36½°. Now the ray A C after reflexion in the direction C S, without any refraction at B, where it emerges perpendicularly to M *d*, would also have had the inclination of its planes of polarization equal to 36½° if there had been no intermediate refraction at A; but this refraction alone being

Fig. 3.



capable of producing an inclination of 53° or a rotation of 53° − 45° = 8°, and this rotation being in an opposite direction from that produced by the second reflexion at C, the inclination of the planes of polarization for the ray C S is nearly 44½°, the reflexion at C having brought back the ray A C almost exactly into the state of natural light.

Without changing either the light or the angle, I cemented a prism M*cd* on the face M*d*, so that *c d* was parallel to *d* N, and I found that the second refraction at *b*, equal to that at A, changed the inclination of the planes of polarization to 53°; that is, the two refractive actions at A and *b* had overcome the action of reflexion at C, and the pencil *bs* actually contained light polarized perpendicular to the plane of reflexion.

In order to put this result to another test, I took a plate M *c* N Q (Fig. 3.) of the same glass, which separated the pencil *b s* reflected at the second surface, from the parallel pencil A P reflected from the first surface, and I found that at an angle of 83°, the value of the inclination I or φ for the ray was about 37½°, while the value of I for the ray *b s* was nearly 55°, an effect almost equal to the refractive action of a plate at 83° of incidence.

When the pencil R A is incident on the first surface at the polarizing angle or 56° 45′, the rotation produced by refraction at A is about 2°, or the inclination I = 45° + 2° = 47°; but the maximum action of the polarizing force at C is sufficient to make I = 0° whether x is 45° or 47°. Hence CB is completely polarized in the plane of reflexion, and the refractive action at *b* is incapable of changing the plane of polarization when I = 0°: the reason is therefore obvious why the two rotations at A and *b*, of 2° each, produce no effect at the maximum polarizing angle.

If we now call

φ = Inclination to the plane of reflexion produced by the 1st refraction at A,

φ' = Inclination produced by the reflexion at C,

φ'' = Inclination produced by the 2nd refraction at *b*,

We shall have

$$\text{Cot}\,\varphi = \cos\,(i - i')\,;\ \text{or}\ \tan\varphi = \frac{1}{\cos\,(i - i')}$$

$$\text{Tan } \varphi' = \tan x \left(\frac{\cos(i+i')}{\cos(i-i')} = \frac{\cos(i+i')}{(\cos(i-i'))^3}\right.$$

$$\text{Cot } \varphi'' = \cot x \left(\cos(i-i')\right) = \frac{(\cos(i-i'))^3}{\cos(+i')}$$

These formulæ are suited to common light where $x = 45°$, but when x varies they become

$$\text{Cot } \varphi = \cot x \left(\cos(i-i')\right)$$

$$\text{Tan } \varphi' = \tan x \left(\frac{\cos(i+i')}{(\cos(i-i'))^3}\right)$$

$$\text{Cot } \varphi'' = (\cot x \left(\frac{(\cos(i-i'))^3}{\cos(i+i')}\right).$$

Resuming the formula for common light, viz. $\cot \varphi'' = \frac{(\cos(i-i'))^3}{\cos(i+i')}$, it is obvious that when $(\cos(i-i'))^3 = \cos(i+i')$, $\cot \varphi'' = 1$, and $\varphi'' = 45°$; that is, the light is restored to common light.

In glass where $m = 1.525$ this effect takes place at 78° 7′; a little below 78° in diamond; and a little above 80° in water.

At an angle below this, φ becomes less than 45°, and the pencil contains light polarized in the plane of reflexion; while at all greater angles φ is above 45°, and the pencil contains light polarized perpendicular to the plane of reflexion. Hence we obtain the following curious law.

"A pencil of light reflected from the second surfaces of transparent plates, and reaching the eye after two refractions and an intermediate reflexion, contains at all angles of incidence from 0° to the maximum polarizing angle, a portion of light polarized in the plane of reflexion. Above the polarizing angle the part of the pencil polarized in the plane of reflexion diminishes till $\cos(i+i') = (\cos(i-i'))^3$, when it disappears, and the whole pencil has the character of common light. Above this last angle the pencil contains a quantity of light polarized perpendicularly to the plane of reflexion, which increases to a maximum and then diminishes to zero at 90°."

Let us now examine the state of the pencil CS′ that has suffered only one refraction and one reflexion. Resuming the formula $\tan \varphi' = \frac{\cos(i+i')}{(\cos(i-i'))^3}$, it is evident that when $(\cos(i-i'))^2 = \cos(i+i')$, $\varphi' = 45°$, and consequently the light is restored to common light. This takes place in glass at an angle

of 82° 44′. At all angles beneath this the pencil contains light polarized in the plane of reflexion; but at all angles above it, the pencil contains light polarized perpendicular to the plane of reflexion, the quantity increasing from 82° 44′ to its maximum, and returning to its minimum at 90°.

By comparing these deductions with the formula and table for reflected light given in my paper On the Laws of the Polarization of Light by Refraction, the following approximate law will be observed. When

$\text{Cos}\,(i - i') = \cos(i + i')$	All the incident light is reflected.
$(\text{Cos}\,(i - i'))^2 = \cos(i + i')$	Half the incident light is reflected.
$(\text{Cos}\,(i - i'))^3 = \cos(i + i')$	A third of the incident light is reflected.
$(\text{Cos}\,(i - i'))^n = \cos(i + i')$	An nth part of the incident light nearly is reflected.

This law deviates from the truth by a regular progression as n increases, and always gives the value of the reflected light in defect. Thus

Angles of Incidence.	Values of n.	Differences.
82° 44′	2	0
78 34	3	12
75 38	4	21
68 56	8	38
66 4	11	43
61 22	20	50

Let us now apply the results of the preceding analysis to M. Arago's experiment shown in Fig. 1. Suppose the angle of incidence to be 78° 7′, and let the light polarized by reflexion at A (Fig. 3.) be $= m$, and that polarized by one refraction also $= m$. Then since the pencil bs is common light, the polarized light in the whole reflected pencil AP, bs is $= m$, whereas the light polarized by the two refractions is $= 2m$; so that M. Arago's experiment makes two quantities appear equal when the one is double that of the other. If the angle exceeds 78° 7′, the oppositely polarized light in the pencil bs will neutralize a portion of the polarized light in the pencil AP, and the ratio of the oppositely polarized rays which seem to be compensated in the experiment, may be that of $3m$ or even $4m$ to 1.

Having thus determined the changes which light undergoes by reflexion

from plates, it is easy to obtain formulæ for computing the exact quantities of polarized light at any angle of incidence, either in the pencil CBS or *bs*.

The primitive ray RA being common light, AC will not be in that state, but will have its planes of polarization turned round a quantity x by the refraction at A; so that $\cot x = \cos(i - i')$. Hence we must adopt for the measure of the light reflected at C the formula of FRESNEL for polarized light whose plane of incidence forms an angle x with the plane of reflexion. The intensity of AC being known from the formula for common light, we shall call it unity, then the intensity I of the two pencils polarized $-x$ and $+x$ to the plane of reflexion will be

$$I = \frac{\sin^2(i - i')}{\sin^2(i + i')} \cos^2 x + \frac{\tan^2(i - i')}{\tan^2(i + i')} \sin^2 x \quad \text{and}$$

$$Q = I \left(1 - 2 \frac{\left(\frac{\cos(i + i')}{(\cos(i - i'))^3} \right)^3}{+1 \left(\frac{\cos(i + i')}{(\cos(i - i'))^3} \right)^2} \right)$$

In like manner if we call the intensity of CB = 1, we shall have

$$\text{Tan } x = \frac{\cos(i + i')}{(\cos(i - i'))^3}$$

and the intensity I of the transmitted pencil *bs*

$$I = 1 - \frac{\sin^2(i - i')}{\sin^2(i + i')} \cos^2 x + \frac{\tan^2(i - i')}{\tan^2(i + i')} \sin^2 x \quad \text{and}$$

$$Q = \left(I\ 1 - 2 \frac{\left(\frac{(\cos(i - i'))^3}{\cos(i + i')} \right)^3}{1 + \left(\frac{(\cos(i - i'))^3}{\cos(i + i')} \right)^2} \right)$$

I shall now conclude this paper with the following Table computed from the formulæ in pages 148, 149, and showing the state of the planes of polarization of the three rays AC, CS, and *bs*.

Angle of Incidence on the First Surface.	Angle of Refraction at First Surface, and Angle of Incidence on Second Surface.	Inclination of Plane of Polarization of A C Fig. 3.	Inclination of Plane of Polarization of C S Fig. 3.	Inclination of Plane of Polarization of *b s* Fig. 3.
° ′	° ′	° ′	° ′	° ′
0 0	0 0	45 0	45 0	45 0
32 0	20 33	45 34	32 20	32 51
40 0	25 10	45 58	24 12	24 56
45 0	27 55	46 17	17 49	18 38
56 30	33 30	47 22	0 0	0 0
67 0	37 34	48 57	18 20	20 50
70 0	38 30	49 33	23 34	27 6
75 0	39 46	50 45	32 22	37 48
78 37	40 29	51 49	38 10	44 59
79 0	40 33	51 56	38 49	45 46
80 0	40 42	52 16	40 27	47 46
83 0	41 5	53 21	44 39	53 40
86 30	41 23	54 47	50 58	60 13
90 0	41 58	56 29	56 29	66 19

Allerly, December 31*st*, 1829.

XI. *Observations made with the invariable pendulum* (*No.* 4. JONES), *at the Royal Observatory, Cape of Good Hope, for the purpose of determining the compression of the earth. By the* REV. FEARON FALLOWS, F.R.S. *Astronomer of the Cape Observatory. Communicated by the Lords Commissioners of the Admiralty.*

Read February 18, 1830.

THE important problem of ascertaining the ellipticity of the earth has, for a long time past, drawn forth the talents and labours of some of the most scientific men in Europe; and it still continues to be deemed an object of especial regard by all who feel an interest in the promotion of natural knowledge. To attempt to depict the strenuous exertions, the innumerable fatigues, the ardour for the improvement of science, which actuated so many illustrious persons in endeavouring to discover the true figure of the earth, would only be a waste of time; as learned Societies have always recognised and stamped the due meed of merit to each, and invariably appreciated and published to the world the valuable results which have with so many difficulties been obtained.

The nations of Europe, emulous of each other in a work which particularly distinguishes the acquirements of modern times, have encouraged and laudably given their protection and sanction to eminent individuals engaged in undertakings so conducive to the honour of an enlightened community.

Whatever difference of opinion may exist as to the respective values to be attached to observations in the admeasurement of an arc of the meridian, or by the variation in the lengths of the seconds pendulum made in different latitudes, there can be no doubt that the former has this paramount advantage; that certain fixed points are determined, by which the geography of a country is considerably promoted, and at length brought as nearly as possible to perfection: whilst the latter method possesses a superiority in being able to concentrate under the immediate eye of the observer the results

of his inquiries, which are readily obtained, and easily performed and accomplished.

The Lords Commissioners of the Admiralty having furnished this observatory with the invariable pendulum (No. 4. Jones), which had for several years been most strictly examined by Captain Sabine at Mr. Browne's, in Portland Place, London, and subsequently by the same learned gentleman in the course of his inquiries in different parts of the earth, I wished at the earliest opportunity to have a series of experiments made upon it, which might unite every possible degree of accuracy on the part of the observers, as well as the utmost stability in the structure intended for supporting the pendulum. From many unforeseen circumstances, I found at length that the completion of this observatory would be delayed for a considerable time, though previously I had, too fondly perhaps, anticipated finishing it at a much earlier period; and I therefore took advantage of a small out-house (then of no further use) within a short distance of the building, to erect a stout brick pier, well bonded, for the support of the small transit instrument; the same which I used in forming my catalogue of 273 southern stars reduced to the beginning of 1824.

Temporary chases were made for meridian observations; and the recess for the clock (Molyneux's *) and pendulum was soon constructed in a most substantial manner within a few feet of the transit. The top of this recess consisted of a thick strong Robben-Island stone, perforated in the middle, and chiselled out at the upper part, for the reception of the brass plate containing the agate planes upon which the knife-edges of the pendulum rest. This plate was securely fastened to the stone with cement; and I found that when the agates were once truly adjusted to level, they remained (with one exception only, and I think this might be attributed to some error in one of the small levels,) during the whole of the observations that succeeded. It is hardly worthy of remark that at the end of each day's work the knife-edges were screwed up from the agates, and remained so till the commencement of another series on the first favourable opportunity. The clock (adjusted to sidereal time and corrected for rate by the stars,) was placed in the recess, having the bottom of the case resting upon a large block of stone embedded in well-wrought clay, and the back of the case was tightly screwed to a strong piece of well seasoned

* The new clock, not the old one.

wood let into the wall. I have little doubt but that the good going of the clock was by these means in a great measure secured. On each side of the pendulum, somewhat above the middle of it, were suspended two of Jones's thermometers. The disk was formed of a small circle of card paper about three-tenths of an inch in diameter, this measure being found to be the best and most convenient. Instead of the wooden stand for supporting the small telescope, through which the coincidences, or rather the disappearances and re-appearances of the pendulum-rod and the disk are noted, as well as the magnitude of the arc of vibration, a small thick brick pier was raised so as to receive the plate to which the telescope is attached. The usual adjustments of the clock, pendulum and telescope, being completed, we were now ready to commence.

I here deem it right to notice the very able assistance which I received from Captain Ronald: his exertions were truly praiseworthy; I cannot recommend them too highly. I likewise avail myself of paying a just tribute to the aid which was afforded by Lieutenant Johnson, of the Honourable East India Company's Service, now superintendant of the observatory at St. Helena. This gentleman being on a visit to me, very kindly at my request took an active part in all the observations.

The sheets accompanying this short paper must be considered as the united labours of Captain Ronald, Mr. Johnson, and myself, and the responsibility of each as to accuracy must depend upon the papers signed with the observer's name. As far as I am able to judge of these things, the near agreement of three independent series of observations is no small argument of their accuracy.

It must not for a moment be conceived that I reckon our results as in any way final in the determination of the compression of the earth, inasmuch as it would be advisable to begin a new series of observations (say) in the midst of winter, or what would be better, in different seasons of the year, lest the coefficient for temperature might require some correction; though even this test can hardly be supposed, from the experiments made by Captain Sabine on this very pendulum in London, to be required. I have ever been of opinion, (how far correct or not, I leave others to judge,) that the invariable pendulum ought

to be a standard instrument in an observatory; that it should be swung at all seasons of the year; that it would be proper to forward it on authorized occasions to the various fixed observatories now situated in the northern and southern hemispheres; that the instrument should return again to the same stations as before, and the observations be renewed; that, finally, after each circuit it should undergo a strict examination at the spot where it was first tried, in order that it might proceed again as before. Should any harm take place from improper packing or accidental circumstances, the evil would be soon discovered, and the instrument repaired. The pendulum is of a very delicate construction, and consequently it is the more necessary that it should be as often as possible compared at those points where it has previously been used.

Formulæ used in computing the Observations.

a = greater, and b the lesser arc observed at the beginning and end of each set of coincidences.
t = mean of the thermometers immediately adjacent to the pendulum.
H = height of the barometer.
τ = height of the attached thermometer.

It must be remarked that the sidereal day is reduced to a mean solar day for comparison in London.

$$\text{Log (reduct. for arc)} = 9.55132 + \log(a+b) + \log(a-b) - \log\{\log a - \log b\}$$

$$\text{Reduct. for temp.} = (t - 62^\circ) \times 0.421.$$

$$\left.\begin{aligned}\text{Log (reduct. for vacuum)} &= 9.31083 + \log \text{H.} \\ &- \{\log(1 + \overline{\cdot 002083} \,.\, \overline{t - 53^\circ}) + \log(1 + \overline{\cdot 0001} \,.\, \overline{\tau - 53^\circ}\}\end{aligned}\right\}$$

The specific gravity of the pendulum is assumed . . .	8.6	Sabine.
The expansion for 1° of the pendulum	0.421	—
The temperature assumed as the standard for spec. grav. =	53°	—
The temperature assumed as the standard for pendulum =	62°	—
Specific gravity of the air (water = 1)	$\frac{1}{770}$	—

The height of barometer assumed as standard for spec. grav. = 29.27 inches.

The height of the pendulum above the mean level of the sea in Table Bay was found to be 32 feet nearly, and above the low-water mark 34 feet, on the day when it was determined. Hence

	Vibrations.
For 34 feet at the Cape	Reduction = 0.14
For 83 feet in London	Reduction = 0.34
	Difference = 0.20

This quantity 0.20 vibration is additive to the difference between the number of vibrations (in a mean solar day) at Mr. Browne's and the Cape.

Brief statement of the final results.

1st,—Captain Ronald.

Observations made in London.—Pendulum (No. 4. Jones).

	Vibrations in a Mean Solar Day.
Mean of 100 coincidences . . .	= 86164.62

Observations made at the Cape of Good Hope.

	Vibrations in a Mean Solar Day.
Mean of 270 coincidences	= 86097.64
Mean of 160 ——	= 86097.88
Mean of 200 ——	= 86097.74
True Mean of 630 ——	= 86097.73

2nd,—Mr. Johnson.

	Vibrations in a Mean Solar Day.
Mean of 240 coincidences	= 86097.792
Mean of 180 ——	= 86097.898
True Mean of 420 ——	= 86097.83

3rd,—Mr. Fallows.

		Vibrations in a Mean Solar Day.
Mean of 401	coincidences	= 86097.67
Mean of 419	——	= 86097.66
Mean of 507	——	= 86097.70
Mean of 81	——	= 86097.69
True Mean of 1408	——	= 86097.68

Summary at the Cape.

			Vibrations in a Mean Solar Day.
Captain Ronald. .	True Mean of	630 coincidences	= 86097.73
Mr. Johnson . . .	True Mean of	420 ——	= 86097.83
Mr. Fallows . . .	True Mean of	1408 ——	= 86097.68
	True Mean of	2458 ——	= 86097.72

From this last conclusion, compared with that obtained by Captain Sabine at Mr. Browne's, in Portland Place, and which only differs $\frac{1}{10}$th of a vibration in a day from Captain Ronald's subsequent determination, the compression of the earth is very readily ascertained.

Let n = the difference in the number of vibrations of the pendulum at London and the Cape. Then for

Compression $\left\{\begin{array}{l} \frac{1}{\cdots} \ldots\ldots\ n = 65.752 \\ \frac{1}{\cdots} \ldots\ldots\ n = 67.344 \\ \frac{1}{\cdots} \ldots\ldots\ n = 68.828 \end{array}\right.$

London. No. of vibrations in a mean solar day . = 86164.64 Sabine.
Cape. No. of vibrations in a mean solar day . = 86097.72

66.92
Reduction due to difference of level from low-water mark = 0.20

True difference in the No. of Vib^{ns} in a mean solar day . = 67.12

This quantity gives the compression of the earth . . = $\frac{1}{\cdots}$

I am aware that this compression somewhat differs from that obtained in the Southern Hemisphere by Sir Thomas Brisbane, at Paramatta, though it is nearly in accordance with M. Freycinet's observations in Cape Town. The documents, however, are here given, and I have every hope that they may meet the public eye, and undergo the usual test of candid and liberal criticism.

Fearon Fallows.

Royal Observatory, Cape of Good Hope,
May 19th, 1829.

P. S. The latitude of the Observatory is not yet ascertained by actual observation with the new mural circle. This instrument has been attached to its pier for several months past; but from some discrepancies in reading, the cause of which is not yet discovered, I am under the necessity of assuming 33° 55′ 56″ as a very near approximate latitude*. The examination of the mural will go on, and the conclusions be forwarded home when some definite opinion may be formed of it.

* I connected the Observatory with my former temporary one in Cape Town, by a survey over the intermediate ground.

Appendix.

Observations made by Captain Ronald with the invariable pendulum (No. 4. of Sabine), in Portland Place, London. Lat. 51° 31′ 8″.4.

1825.	Barom.	Therm.	No. of Coin.	Disapp.	Re-app.	Time of Coincidence.	Arc.
	inches.	°		m s	m s	h m s	°
July 31	29.950	70.48	1	31 21	31 28	1 31 24.5	1.185
	29.934	70.50	11	28 03	28 18	3 28 10.5	.610
Aug. 1	29.944	69.75	1	13 35	13 45	11 13 40.0	1.110
	29.942	70.25	11	10 34	10 49	1 10 41.5	.575
"	29.942	70.35	1	33 55	34 01	1 33 58.0	1.320
	29.941	70.65	11	30 31	30 45	3 30 38.0	.670
2	29.950	71.00	1	49 29	49 36	10 49 32.5	1.225
	29.944	71.90	11	45 59	46 15	0 46 7.0	.640
"	29.944	71.90	1	58 01	58 08	0 58 4.5	1.305
	29.944	71.80	11	54 26	54 41	2 54 33.5	.665
3	29.952	71.00	1	9 39	9 44	11 9 41.5	1.370
	29.952	71.80	11	6 02	6 14	1 6 8.0	.695
"	29.953	71.80	1	18 47	18 53	1 18 51.0	1.180
	29.924	72.00	11	15 16	15 29	3 15 22.5	.605
4	29.584	72.20	1	52 51	52 57	0 52 54.0	1.310
	29.574	72.00	11	49 11	49 24	2 49 17.5	.665
5	29.500	70.30	1	33 38	33 44	10 33 41.0	1.290
	29.520	70.75	11	30 18	30 30	0 30 24.0	.640
"	29.522	70.80	1	36 24	36 31	0 36 27.5	1.370
	29.530	71.05	11	32 51	33 03	2 33 57.0	.685
6	29.628	67.20	1	46 11	46 16	11 46 13.5	1.315
	29.616	68.60	11	43 21	43 34	1 43 27.5	.650
Mean..	29.804						

Observations computed.

Observations.	Mean Temp.	Interval.	Vib^{ns} in 24 Mean Solar Hours.	Corr. for Arc.	Reduct. to 70° 82′ Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours at 62° Fahr.
		h m s				
1	70.49	1 56 46.0	86153.354	+1.277	−0.139	86154.492
2	70.00	1 57 01.5	86153.899	1.124	0.345	86154.678
3	70.50	1 56 40.0	86153.143	1.568	0.135	86154.576
4	71.45	1 56 34.5	86152.949	1.378	+0.265	86154.592
5	71.85	1 56 29.0	86152.754	1.534	0.434	86154.722
6	71.40	1 56 26.5	86152.666	1.685	0.244	86154.595
7	71.90	1 56 31.5	86152.843	1.260	0.455	86154.558
8	72.10	1 56 23.5	86152.560	1.541	0.539	86154.640
9	70.52	1 56 43.0	86153.249	1.467	−0.126	86154.590
10	70.92	1 56 29.5	86152.772	1.665	+0.042	86154.479
11	67.90	1 57 14.0	86154.336	+1.521	−1.229	86154.629
Mean =	70.82					86154.595
				Correction for rate.....		+0.441
				Correction for buoyancy....		+5.873
				Reduction to 62° Fahr.....		+3.713
						86164.622*

* Note.—The accordance of this result with that obtained by Captain Sabine, is sufficient to show that the instrument remained unaltered, and warrants the comparison of any future observations with those obtained by former observers; subject, of course, to any change, which it may have undergone since that period, or that it may yet meet with, which can only be satisfactorily ascertained, when another series shall have been made with it, in London, where its rate is probably better known, than at any other of Captain Sabine's stations.

Mr. Browne, at whose house these observations were made, favoured me with the rate of the clock, as follows:

1825.		Cumming with Molyneux set =		Molyneux with Mean Time.
		s		s
July 31	12 p.m.	+0.2		July 30th, fast 0.26
August 1	noon	0.4	+ 0.2	Aug. 6th, ... 0.55
	12 p.m.	0.6	0.2	
2	noon	0.8	0.2	÷)0.29
	12 p.m.	1.0	0.2	0.042 per diem.
3	noon	1.2	0.2	Cumming gains 0.400
	12 p.m.	1.4	0.2	
4	noon	1.6	0.2	Gains on Mean Time.. 0.442=rate.
	12 p.m.	1.8	0.2	
5	noon	2.0	0.2	
	12 p.m.	2.2	0.2	
6	noon	+2.4	+ 0.2	

Observations made with the pendulum (No. 4, of Sabine). Observatory, Cape of Good Hope. Lat. 33° 55′ 56″. By Captain Ronald.

1828.	Barometer.	Thermometers. A.	Thermometers. B.	No. of Coin.	Disapp.	Re-app.	Time of Coincidence.	Arc.
	inches.	°	°		m s	m s	h m s	°
Nov. 23	29.906	72.2	73.2	1	4 53	5 9	12 5 1.0	1.03
	29.917	74.7	75.7	31	42 5	42 17	14 42 11.0	.48
	29.917	75.7	74.6	1	50 2	50 15	14 50 8.5	1.11
"	29.947	71.3	72.3	31	27 13	27 26	17 27 19.5	.51
24	29.956	67.8	68.4	1	56 22	56 35	12 56 28.5	1.215
	29.983	71.2	71.8	31	33 57	34 9	15 34 3.0	.520
	29.986	70.2	70.9	1	14 13	14 27	16 14 20.0	1.27
"	29.951	69.3	69.9	31	51 50	52 2	18 15 56.0	.52
	29.951	69.3	69.9	1	57 45	57 59	18 57 52.0	1.27
"	29.964	68.1	68.9	31	35 23	35 36	21 35 29.5	.53
	29.966	66.3	67.2	1	55 49	56 1	9 55 55.0	1.175
25	29.998	67.5	68.2	31	33 44	33 59	12 33 51.5	.495
	29.998	67.5	68.1	1	41 20	41 33	12 41 26.5	1.32
"	30.006	70.6	71.4	31	19 5	19 17	15 19 11.0	.54
	29.997	72.7	73.4	1	25 52	26 8	16 26 0.0	1.200
"	29.966	72.3	73.0	31	3 8	3 22	19 3 15.0	.515
	29.963	71.7	72.6	1	19 7	19 21	19 19 14.0	1.20
"	29.966	71.9	72.8	31	56 31	56 44	21 56 37.5	.50
Mean..	29.963							

Observations computed. The clock making on the 23rd, 86636.826*, on the 24th, 86636.916*, and on the 25th, 86637.024* sidereal vibrations in a mean solar day.

Number of Observations.	Mean Temp.	Interval.	Vib^{ns} in 24 Mean Solar Hours.	Correc. for Arc.	Reduc. to 62° Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours at 62° F.
	°	h m s				
1	73.95	2 37 10.0	86085.584	+0.891	+5.031	86091.506
2	73.47	2 37 11.0	86085.643	1.024	4.829	86091.496
3	69.80	2 37 34.5	86087.102	1.164	3.284	86091.550
4	70.02	2 37 36.0	86087.189	1.232	3.376	86091.794
5	69.05	2 37 37.5	86087.276	1.249	2.968	86091.493
6	67.30	2 37 56.5	86088.486	1.077	2.231	86091.794
7	69.40	2 37 44.5	86087.790	1.329	3.115	86092.234
8	72.85	2 37 15.0	86086.073	1.138	4.568	86091.779
9	72.25	2 37 23.5	86086.569	+1.114	+4.315	86091.998
Mean ..	70.90				Mean....	86091.738
					Correction for buoyancy	+5.900
		Number of Vibrations in vacuo, in 24 Mean Solar Hours..				86097.638

Rate of the Sidereal Clock.

Nov. 22nd.			Nov. 24th.			Nov. 25th.		
Stars.	Rates.		Stars.	Rates.		Stars.	Rates.	
	s			s			s	
α Pegasi	+0.10	Compared with Observations of the 19th and 21st.	α Arietis ..	+0.10	Compared with Observations of the 22nd.	α Pegasi	+0.35	Compared with Observations of the 22nd and 24th.
α Andromedæ	.07		α Ceti	.29		α App. Sc. ..	.40	
γ Pegasi	.04		γ Eridani..	.42		β Arietis	.45	
β Arietis	.12		Aldebaran .	.35		α ——.....	.24	
α ——.....	.13					ρ Ceti.....	.49	
γ Ceti	.17		Mean..	+.290		δ ——.....	.36	
α ——.....	.28					γ ——.....	.43	
γ Eridani....	.37					α ——.....	.69	
Aldebaran ..	+.23					γ Eridani....	.55	
						Aldebaran ..	+.63	
Mean..	+.168					Mean..	+.459	

Here the rate appears to have been increasing, and therefore a proportion was made for each day's observations, thus:

	s
For the observations of the 23d, Rate	+0.270
24th, . . .	+.360
25th, . . .	+.468

* Note.—In these numbers, both in this and the two following series, the rate of the clock is included, being = 86636.556 + rate.

Observations with the pendulum (Continued).

1829.	Attached Therm.	Barom.	Thermometers. A.	Thermometers. B.	No. of Coin.	Disapp.	Re-app.	Time of Coincidence.	Arc.
	°	inches.	°	°		m s	m s	h m s	
Jan. 7	72.0	30.035	72.4	73.0	1	51 16	51 31	15 51 23.5	1.12
	73.5	30.020	73.0	73.6	21	36 20	36 34	17 36 27.0	.62
	77.5	29.964	76.2	76.8	1	34 22	34 36	21 34 29.0	1.18
"	77.0	29.919	74.8	75.2	21	19 10	19 25	23 19 17.5	.66
	77.5	29.919	75.6	76.0	1	30 4	30 18	23 30 11.0	1.37
"	76.5	29.900	74.5	75.1	21	14 46	15 2	1 14 54.0	.73
8	69.0	29.952	69.0	69.6	1	45 56	46 11	15 46 3.5	1.30
	69.6	29.951	70.6	71.1	21	31 3	31 18	17 31 10.5	.71
	72.0	29.951	70.6	71.2	1	36 41	36 56	17 36 48.5	1.26
"	72.5	29.932	72.0	72.6	21	21 45	21 58	19 21 51.5	.69
	72.5	29.932	72.0	72.7	1	27 44	27 58	19 27 51.0	1.23
"	74.0	29.925	72.6	73.4	21	12 43	12 58	21 12 50.5	.66
	74.0	29.925	72.3	73.0	1	24 10	24 22	21 24 16.0	1.49
"	73.0	29.926	72.4	73.1	21	8 59	9 15	23 9 7.0	.83
	73.0	29.926	72.3	72.9	1	14 46	15 2	23 14 54.0	1.19
"	73.0	29.918	70.9	71.5	21	59 45	0 0	0 59 52.5	.66
Mean..	73.54	29.943							

On the morning of the 9th the clock was found stopped, which prevented the continuance of this series.

Observations computed. The clock making on the 7th, 86636.306, and on the 8th, 86636.215 sidereal vibrations in a mean solar day.

Number of Observations.	Mean Temp.	Interval.	Vib^{ns} in 24 Mean Solar Hours.	Correc. for Arc.	Reduc. to 62° Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours at 62° F.
	°	h m s				
1	73.00	1 45 3.5	86086.539	+1.206	+4.631	86092.376
2	75.75	1 44 48.5	86085.228	1.349	5.789	86092.366
3	75.30	1 44 45.5	86084.746	1.794	5.599	86092.139
4	70.08	1 45 7.0	86086.756	1.607	3.402	86091.765
5	71.60	1 45 3.0	86086.408	1.513	4.042	86091.963
6	72.67	1 44 59.5	86086.058	1.418	4.492	86091.968
7	72.70	1 44 51.0	86085.359	2.145	4.505	86092.009
8	71.90	1 44 58.5	86086.015	+1.363	+4.168	86091.546
Mean ..	72.88				Mean....	86092.016
					Correction for buoyancy.....	+ 5.866
Number of Vibrations in vacuo, in 24 Mean Solar Hours....						86097.882

Rate of the Sidereal Clock.

Jan. 7th. Stars.	Rates.		Jan. 8th. Stars.	Rates.	
	s			s	
α Ceti	—0.32	Compared with Observations of the 6th.	α Ceti	—0.40	Compared with Observations of the 6th & 7th.
α Persei....	.10		γ Eridani..	.43	
γ Eridani ..	.30		χ Cali Sc...	.10	
o ——— ..	.28		Capella	.50	
X ——— ..	.04		Rigel.....	.30	
43d ——— ..	.40		γ Orionis ..	.20	
ψ ——— ..	.30		ζ ——— ..	.30	
γ Cali Sc. ..	.47		β Aurigæ ..	— .50	
γ Orionis ..	.52				
δ ——— ..	.08		Mean	— .341	
ε ——— ..	.28				
ζ ——— ..	.12				
β Aurigæ ..	— .04				
Mean	— .250				

Observations with the pendulum (Continued).

1829.	Attached Therm.	Barom.	Thermometers. A.	Thermometers. B.	No. of Coin.	Disapp.	Re-app.	Time of Coincidence.	Arc.
	°	inches.	°	°		m s	m s	h m s	
Jan. 16	78.0	29.986	80.1	80.8	1	57 22	57 34	20 57 28.0	1.41
	78.0	29.998	73.2	73.7	21	41 55	42 9	22 42 2.0	.77
	78.0	29.998	74.0	74.6	1	47 16	47 30	22 47 23.0	1.30
"	76.0	29.990	72.7	73.2	21	32 2	32 16	0 32 9.0	.71
	76.0	29.990	72.8	73.3	1	37 44	37 58	0 37 51.0	1.11
"	76.0	29.990	72.4	72.9	21	22 40	22 55	2 22 47.5	.59
	70.0	29.988	71.0	71.7	1	32 32	32 44	13 32 38.0	1.46
17	72.0	30.014	71.3	71.8	21	17 32	17 47	15 17 39.5	.73
	72.0	30.014	71.3	71.8	1	20 39.5	20 54	15 20 46.25	1.42
"	74.0	30.026	71.8	72.5	21	5 39	5 55	17 5 47.0	.75
	74.0	30.026	72.0	72.7	1	9 4	9 18	17 9 11.0	1.22
"	75.5	30.025	72.6	73.1	21	54 6	54 21	18 54 13.5	.64
	75.6	30.025	73.0	73.5	1	57 1	57 14	18 57 7.5	1.24
"	75.6	30.023	72.9	73.4	21	41 59	42 13	20 42 6.0	.70
	75.6	30.023	73.6	74.1	1	45 54	46 8	20 46 1.0	1.35
"	76.0	30.006	73.1	73.7	21	30 47	31 0	22 30 53.5	.72
	76.0	30.006	74.0	74.6	1	35 49	36 2	22 35 55.5	1.30
"	75.0	29.990	73.3	73.8	21	20 43	20 57	0 20 50.0	.70
	75.0	29.990	73.6	74.8	1	24 34	24 48	0 24 41.0	1.44
"	72.0	29.988	73.0	73.6	21	9 25	9 40	2 9 32.5	.74
Mean..	75.01	30.005							

Observations computed. The clock making on the 16th, 86636.057, and on the 17th, 86636.027 sidereal vibrations in a mean solar day.

Number of Observations.	Mean Temp.	Interval.	Vib^{ns} in 24 Mean Solar Hours.	Corr. for Arc.	Reduc. to 62° Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours at 62° F.
	°	h m s				
1	76.95	1 44 34.0	86083.707	+1.890	+6.294	86091.891
2	73.62	1 44 46.0	86084.762	1.507	4.892	86091.261
3	72.85	1 44 56.5	86085.681	1.146	4.568	86091.395
4	71.45	1 45 1.5	86086.088	1.890	3.978	86091.956
5	71.85	1 45 0.75	86086.022	1.867	4.147	86092.036
6	72.60	1 45 2.5	86086.175	1.370	4.463	86092.008
7	73.20	1 44 58.5	86085.826	1.501	4.715	86092.042
8	73.62	1 44 52.5	86085.301	1.700	4.892	86091.893
9	73.92	1 44 54.5	86085.476	1.589	5.018	86092.083
10	73.75	1 44 51.5	86085.214	+1.879	+4.947	86092.040
Mean ..	73.38				Mean....	86091.860
					Correction for buoyancy.....	+ 5.876
		Number of Vibrations in vacuo, in 24 Mean Solar Hours.....				86097.736

Rate of the Sidereal Clock.

Jan. 16th.			Jan. 17th.		
Stars.	Rates.		Stars.	Rates.	
	s			s	
12 Eridani..	−0.67	Compared with Observations of the 15th.	α Persei ..	−0.60	Compared with Observations of the 15th and 16th.
17 ——..	.40		17 Eridani..	.50	
δ ——..	.52		19 ——..	.50	
ζ Orionis ..	.33		δ ——..	.42	
β Aurigæ ..	.70		γ Orionis ..	.65	
ϑ Geminor..	.28		δ ——..	.59	
β Can. Min.	.38		ε ——..	.58	
Castor	.56		ζ ——..	.74	
Procyon....	− .65		μ Columbæ	.60	
			β Aurigæ ..	.33	
			ϑ Leporis ..	.50	
			ε Can. Maj.	.50	
			δ ——	.48	
			ϑ Geminor.	.57	
			β Can. Min.	.38	
			Procyon....	.47	
			Pollux	− .58	
Mean..	− .499		Mean..	− .529	

Abstract of Captain Ronald's observations.

London.	Mean of 110 coincidences	=	86164.62
Cape.	Mean of 270 coincidences	=	86097.64
——	—— 160	=	86097.88
——	—— 200	=	86097.74
True Mean of 630 coincidences at the Cape		=	86097.73

Cape of Good Hope: Observer, Mr. Johnson.

1828.	Attached Therm.	Barom.	Therm. A.	Therm. B.	No. of Coin.	Disapp.	Re-app.	Time of Coin.	Arc.	Remarks.
Dec.	°	inches.	°			m s	m s	h m s		
15	68.5	30.224	67.3	67.7	1	15 48	16 4	15 15 56.0	1.20	The Agate Planes were thrown out of level, and again adjusted before commencing these observations.
	68.7	30.220	67.9	68.2	21	1 9	1 27	17 1 18.0	0.67	
"	68.8	30.220	68.3	68.6	1	22 22	22 38	17 22 30.0	1.28	
	69.5	30.215	68.6	68.9	21	7 41	8 0	19 7 50.5	0.68	
"	69.5	30.215	68.8	69.0	1	24 9	24 27	19 24 18.0	1.07	
	69.5	30.204	68.4	68.7	21	9 31	9 53	21 9 42.0	0.56	
"	69.5	30.204	68.7	69.2	1	20 38	20 54	21 20 46.0	1.32	
	69.0	30.205	68.2	68.7	21	5 53	6 13	23 6 3.0	0.70	
16	69.0	30.118	67.8	68.3	1	50 26	50 43	15 50 34.5	1.20	High wind from S. E. during the whole of this day.
	69.5	30.110	67.3	67.7	21	35 4	36 3	17 35 54.0	0.71	
"	69.5	30.092	67.9	68.3	1	46 45	47 1	17 46 53.0	1.20	
	69.5	30.076	67.7	68.0	21	32 3	32 21	19 32 12.0	0.72	
"	69.5	30.076	68.0	68.4	1	48 27	48 44	19 48 35.5	1.23	
	70.0	30.048	67.9	68.3	21	33 44	34 1	21 33 52.5	0.77	
"	70.0	30.048	68.0	68.4	1	44 45	45 2	21 44 53.5	1.21	
	68.5	30.028	67.8	68.2	21	30 1	30 19	23 30 10.0	0.78	
17	68.5	30.028	67.5	68.0	1	39 34	39 50	14 39 42.0	1.20	
	69.5	30.022	67.8	68.3	21	24 51	25 9	16 25 0.0	0.72	
"	69.5	30.022	68.2	68.7	1	35 45	36 1	16 35 53.0	1.12	
	70.5	30.016	68.5	69.0	21	20 58	21 15	18 21 6.5	0.68	
"	70.5	30.016	69.0	69.5	1	37 33	37 50	18 37 41.5	1.12	
	71.5	30.009	69.5	69.8	21	22 41	22 58	20 22 49.5	0.67	
"	71.5	30.009	68.5	68.8	1	34 1	34 17	20 34 9.0	1.30	
	70.0	30.004	69.0	69.5	21	19 8	19 25	22 19 16.5	0.79	

Observations computed.

No. of Observ.	Mean Temp.	Interval.	Uncorrected Number of Vibrations.	Clock's Rate.	Correc. for Arc.	Reduc. to 62° Fahr.	Red. Vib^{ns} in 24 Mean Solar Hours at 62° Fahr.
	°	h m s		−	+	+	
1	67.78	1 45 22.0	86088.392	0.39	1.394	2.432	86091.828
2	68.50	1 45 20.5	86088.275	0.39	1.524	2.779	86092.188
3	68.73	1 45 24.0	86088.568	0.39	1.052	2.833	86092.063
4	68.70	1 45 17.0	86087.960	0.39	1.622	2.821	86092.013
5	67.78	1 45 19.5	86088.198	0.21	1.461	2.433	86091.882
6	67.97	1 45 19.0	86088.137	0.21	1.482	2.513	86091.922
7	68.15	1 45 17.0	86087.960	0.21	1.609	2.589	86091.948
8	68.10	1 45 16.5	86087.920	0.21	1.597	2.568	86091.875
9	67.90	1 45 18.0	86088.059	0.21	1.482	2.484	86091.815
10	68.63	1 45 13.5	86087.660	0.21	1.300	2.791	86091.541
11	69.45	1 45 8.0	86087.180	0.21	1.284	3.136	86091.390
12	68.95	1 45 7.5	86087.120	0.21	1.753	2.925	86091.588
Mean..	68.39					Mean	86091.838
						Correction for buoyancy	+5.956
						Number of Vibrations in vacuo, in 24 Mean Solar Hours ..	86097.794

Rate of the Sidereal Clock.

Dec. 14th.	s	Dec. 15th.	s
By α Arietis.....	−0.53	By α Ceti	−0.74
Aldebaran	0.26	Aldebaran	0.28
Capella	0.50	Capella	0.50
Rigel	0.12	Rigel	0.14
β Tauri	0.34	β Tauri	0.44
ζ Orionis.....	0.60	δ Orionis.....	0.30
β Aurigæ	0.74	ζ ——.....	0.39
Mean Rate..	−0.44	Mean Rate..	−0.39

Mean Rate on the 18th by Mr. FALLOWS's Observations 0.03

Remark.—On the 16th and 17th the weather was unfavourable for observation; the quantity assumed for the rate of the clock on those days is a mean of its rate on the 15th and 18th: probably on the 17th the rate was less than that which has been assigned.

Observations with the pendulum (Continued).

1829.	Attached Therm.	Barom.	Therm. A.	Therm. B.	No. of Coin.	Disapp.	Re-app.	Time of Coin.	Arc.	Remarks.
	°	inches.	°	°		m s	m s	h m s		
Jan. 20.	68.5	29.972	68.6	69.0	1	9 46	9 55	17 9 50.5	1.67	The Pendulum was not stopped between these Observations.
	73.7	30.017	73.0	73.6	21	54 19	34 35	18 54 27.0	1.12	
	73.7	30.017	72.8	73.4	1	59 34	59 49	18 59 41.5	1.12	
"	71.7	30.010	71.7	72.0	21	44 22	44 37	20 44 29.5	0.83	
	71.7	30.010	71.8	72.0	1	1 55	2 5	21 2 0.0	1.60	The Pendulum was not stopped between these Observations.
"	73.5	30.003	73.0	73.5	21	46 18	46 33	22 46 25.5	1.31	
	73.5	30.003	73.2	73.7	1	51 31	51 45	22 51 38.0	1.31	
"	74.5	30.003	73.5	74.0	21	36 6	36 22	24 36 14.0	0.90	
21.	67.0	30.137	68.8	69.0	1	14 28	14 37	15 14 32.5	1.68	The Pendulum was not stopped between these Observations.
	70.0	30.145	69.4	69.9	21	59 25	59 41	16 59 30.0	0.91	
	70.0	30.145	69.4	69.9	1	4 41	4 57	17 4 49.0	0.90	
"	72.0	30.145	70.0	70.4	21	50 1	50 17	18 50 9.0	0.50	
	72.0	30.145	70.6	71.0	1	7 6	7 17	19 7 11.5	1.60	The Pendulum was not stopped between these Observations.
	73.0	30.137	70.2	70.6	21	52 3	52 17	20 52 10.0	0.87	
	73.0	30.137	70.5	70.8	1	57 18	57 34	20 57 26.0	0.86	
"	72.0	30.116	70.3	70.6	21	43 34	43 49	22 43 41.5	0.48	
	72.0	30.116	70.7	71.3	1	59 40	59 49	22 59 44.5	1.70	
"	71.0	30.114	69.9	70.2	22	49 51	50 9	0 50 0.0	0.87	

Observations computed.

No. of Observ.	Mean Temp.	Interval.	Uncorrected No. of Vibrations.	Clock's Rate.	Correc. for Arc.	Reduc. to 62° Fahr.	Reduced No. of Vib^ns in 24 Mean Solar Hours at 62° Fahr.
	°	h m s					
1	71.05	1 44 36.5	86084.431	+0.54	+3.148	+3.810	86091.929
2	72.50	1 44 48.0	86085.440	+0.54	1.547	4.421	86091.948
3	72.57	1 44 25.5	86083.460	+0.54	3.458	4.454	86091.912
4	73.60	1 44 36.0	86084.392	+0.54	1.978	4.883	86091.793
5	69.27	1 45 0.5	86086.529	−0.410	2.666	3.061	86091.846
6	69.93	1 45 20.0	86088.216	−0.410	0.781	3.338	86091.924
7	70.60	1 44 58.5	86086.373	−0.410	2.425	3.620	86092.008
8	70.55	1 45 15.5	86087.823	−0.410	0.716	3.600	86091.729
9	70.52	1 50 15.5	86086.529	−0.410	+2.609	+3.587	86092.315
						Mean....	86091.934
						Correction for buoyancy.....	+5.944
						Number of Vibrations in vacuo, in 24 Mean Solar Hours ..	86097.878

Rate of the Sidereal Clock.

Jan. 20th.	s	Jan. 21st.	s
By β Tauri.....	+0.44	By β Tauri.....	−0.44
δ Orionis	0.47	δ Orionis	0.40
ζ ———	0.50	ζ ———	0.34
α ———	0.82	α ———	0.48
Procyon	0.64		
Pollux	0.88		
Mean Rate..	+0.54	Mean Rate..	−0.41

Remark.—From the above result, it appears that the rate varied nearly a second in the space of 24 hours: there is reason to suppose that the change was owing to some sudden derangement which the clock might have suffered on the night of the 20th. From the tolerable accordance with each other of the pendulum observations of the 21st, it may be presumed that the rate was equable during that day.

Abstract of Mr. Johnson's observations.

Mean of 240 coincidences . . . . . = 86097.792
Mean of 180 coincidences . . . . . = 86097.878

True mean of 420 coincidences . . . . = 86097.829

General Remark.—During both series of experiments, the observatory was kept in the same state, the North shutter being open during the day, and the door of the recess, in which the clock and pendulum stood, constantly closed, with the exception of the two small openings made in it for the purpose of showing the dial plate, and the graduated arc of the pendulum. During the second series, the weather was particularly favourable, both for observations of the transit instrument and pendulum.

Cape of Good Hope; Observer Mr. Fallows.

First Series.

1829.	Attached Therm.	Barom.	Therm. A.	Therm. B.	No. of Coin.	Disapp.	Re-app.	Time of Coin. by the Sidereal Clock.	Arc.	Remarks.
	°	inches.	°	°		m s	m s	h m s	°	
Dec. 2	66.5	30.205	68.2	69.1	1	50 41	50 56½	16 50 48½	1.38	This day very favourable for observation; little wind—the Observatory chase shut, except when observing. The Thermometers read off before the North chase was opened.
"	67.0	30.205	68.0	68.9	31	28 31	28 51	19 28 41	0.51	
"	67.1	30.205	68.0	68.9	1	33 50	34 4½	19 33 57½	1.48	
"	67.5	30.207	67.1	68.0	31	11 44	12 1	22 11 52½	0.61	
3	66.0	30.205	67.1	68.0	1	38 1	38 16	14 38 8½	1.38	
"	68.5	30.205	69.6	70.5	31	15 56	16 13	17 16 4½	0.57	
"	68.7	30.205	70.1	71.2	1	42 43½	42 59	17 42 51½	1.20	The weather fine during the whole of the day.
"	68.5	30.175	70.2	71.2	31	20 24	20 44½	20 20 34½	0.50	
"	68.6	30.174	70.2	71.3	1	30 6	30 22	20 30 14	1.22	
"	68.0	30.151	68.0	69.0	31	7 57	8 15	23 8 6	0.49	
4	65.0	30.102	64.2	65.0	1	3 18	3 32	12 3 25	1.38	
"	67.5	30.139	66.9	67.6	31	41 31	41 49	14 41 40	0.57	
"	67.6	30.140	66.9	67.6	1	47 24	47 40	14 47 32	1.20	
"	68.5	30.165	69.1	70.0	31	25 26½	25 41½	17 25 34	0.51	The South-east wind springing up. All the doors closed. What is called at Cape Town a South-easter, ought to be designated South-by-east.
"	68.6	30.166	69.1	70.0	1	31 10	31 26	17 31 18	1.35	
"	68.5	30.120	68.3	69.1	31	9 0	9 18	20 9 9	0.58	
"	68.5	30.120	67.0	67.8	1	19 40	19 54	20 19 47	1.41	
"	68.0	30.108	67.3	68.0	31	57 33	57 52	22 57 42½	0.58	
5	64.0	30.125	63.0	63.6	1	40 18	40 34	10 40 26	1.42	The weather rainy; wind due North; no stars last night, from clouds.
"	64.5	30.127	64.0	64.8	31	18 32	18 49	13 18 40½	0.68	
"	64.5	30.127	64.1	64.8	1	24 13	24 28	13 24 20½	1.29	
"	66.4	30.127	65.8	66.4	31	2 31	2 46	16 2 38½	0.56	
"	66.3	30.127	66.3	67.0	1	13 24	13 32	16 13 28	1.72	
"	68.0	30.200	68.4	69.2	42	49 5½	49 22½	19 49 14	0.50	The sky clearing up.
"	68.0	30.200	68.1	68.9	1	54 25	54 41	19 54 33	1.13	
"	68.0	30.115	68.0	68.5	31	32 20	32 36	22 32 28	0.49	
Mean	67.23	30.1594								

First Series computed.

1829.	Mean Temp.	Interval.	Uncorrected No. of Vibrations.	Clock's Rate.	Correc. for Arc.	Reduc. to 62° Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours.
	°	h m s		s			
	68.550	2 37 52¼	86087.78	−0.52	+1.354	+2.758	86091.37
	68.000	2 37 53¼	86087.94	0.52	1.682	2.526	86091.63
	68.800	2 37 56	86087.98	0.41	1.464	2.863	86091.50
	70.540	2 37 43	86087.24	0.41	1.114	3.595	86091.54
	69.625	2 37 52	86087.76	0.41	1.121	3.210	86091.68
	65.925	2 38 15	86089.08	0.26	1.464	1.652	86091.94
	68.400	2 38 2	86088.34	0.26	1.362	2.694	86092.14
	69.125	2 37 51	86087.70	0.26	1.442	3.000	86091.88
	67.525	2 37 55½	86087.98	0.26	1.564	2.326	86091.61
	63.850	2 38 14½	86089.06	0.26	1.730	0.779	86091.31
	65.275	2 38 18	86089.26	0.26	1.326	1.379	86091.69
	67.725	3 35 46	86088.24	0.26	1.797	2.410	86092.15
	68.375	2 37 55	86087.94	−0.26	+1.017	+2.684	86091.38
Mean..	67.824					Mean	86091.69
						Buoyancy	+5.98
(No. of Coincidences = 401)						Number of Vibrations in vacuo, in 24 Mean Solar Hours..	86097.67

Rate of the Sidereal Clock.

Dec. 2nd.		Dec. 3rd.		Dec. 4th to Dec. 9th.	
	s		s		s
By α Arietis.....	−0.38	By α Ceti	−0.24	By α Ceti	−0.33
α Ceti	0.52	Capella	0.30	Capella	0.26
Rigel	0.50	β Tauri	0.50	Rigel	0.22
β Tauri	0.70	δ Orionis.....	0.58	β Tauri	0.23
δ Orionis	0.50	ζ ——.....	0.36	δ Orionis.....	0.23
ζ ——.....	0.55	β Aurigæ	−0.50	β Aurigæ	0.27
β Aurigæ	−0.46			Sirius	−0.28
Mean Rate..	−0.52	Mean Rate ..	−0.41	Mean Rate..	−0.26

Second Series.

1829.	Attached Therm.	Barom.	Thermometers. A.	Thermometers. B.	No. of Coin.	Disapp.	Re-app.	Time of Coincid. by the Sidereal Clock.	Arc.
	°	inches.	°	°		m s	m s	h m s	°
Dec. 18	66.1	30.022	65.2	65.7	1	15 26	15 38	11 15 32	1.78
	69.5	30.053	68.1	68.7	53	49 17	49 35	15 49 26	0.39
	70.0	30.053	71.2	71.7	1	0 9	0 13	16 0 11	1.87
"	72.0	30.016	72.2	72.4	59	4 21	4 37	21 4 29	0.40
	72.1	30.015	72.2	72.4	1	25 47	26 4	21 25 55½	1.12
"	71.0	30.024	71.0	71.5	16	44 30	44 47	22 44 38½	0.76
	71.0	30.025	71.0	71.4	1	55 5	55 22	22 55 13½	0.89
"	71.5	30.054	70.1	70.5	18	24 26	24 42	0 24 34	0.55
	67.5	30.165	68.0	68.3	1	56 39	56 47	12 56 43	1.72
19	72.0	30.185	71.5	72.0	58	16 5	16 18	17 56 11½	0.37
	72.0	30.185	71.6	72.2	1	1 37	1 45	18 1 41	1.79
"	72.5	30.102	70.6	71.2	54	39 43	39 57	22 39 50	0.39
	72.5	30.101	71.2	71.7	1	44 54	45 11	22 45 2½	0.92
"	71.0	30.086	69.8	70.2	20	24 48	25 7	0 24 57½	0.58
	67.5	30.023	67.2	67.8	1	19 24	19 31	11 19 27½	1.77
20	72.0	29.995	73.8	74.2	63	44 56	45 7	16 45 1½	0.34
	72.5	29.994	74.9	74.9	1	51 39	51 47	16 51 43	1.74
"	75.0	29.909	76.8	77.1	52	18 26	18 44	21 18 35	0.44
	75.0	29.908	77.0	77.0	1	30 29	30 46	21 30 37½	1.31
"	74.5	29.901	74.8	75.3	36	33 38	33 55	0 33 46½	0.50
Mean..	71.360	30.036							

Second Series computed.

1829.	Mean Temp.	Interval.	Uncorrected No. of Vibrations.	Clock's Rate.	Correc. for Arc.	Reduc. to 62° Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours.
		h m s					
	66.925	4 33 54	86088.34	−0.03	+1.628	+2.073	86092.04
	71.875	5 4 18	86086.14	0.03	1.757	4.157	86092.02
	71.775	1 18 43	86086.28	0.03	1.427	4.115	86091.79
	70.600	1 29 20½	86087.04	0.03	0.849	3.621	86091.48
	69.950	4 59 28½	86086.90	0.17	1.505	3.347	86091.54
	71.400	4 38 9	86086.30	0.17	1.641	3.957	86091.73
	70.725	1 39 55	86087.40	0.17	0.906	3.673	86091.81
	70.750	5 25 34	86086.59	0.08	1.499	3.684	86091.69
	75.925	4 26 52	86084.68	0.08	1.689	5.862	86092.15
	76.025	3 3 9	86084.68	−0.08	+1.248	+5.905	86091.75
Mean..	71.595					Mean....	86091.76
						Buoyancy.....	+5.90
(No. of Coincidences = 419)						Number of Vibrations in vacuo, in 24 Mean Solar Hours ..	86097.66

Rate of the Sidereal Clock.

Dec. 18th.	s	Dec. 19th.	s	Dec. 20th.	s
By β Aurigæ	−0.04	By α Ceti.....	−0.28	By α Ceti	+0.03
Sirius	−0.15	Aldebaran	0.00	Aldebaran	−0.02
Procyon.....	+0.05	β Tauri	0.00	Rigel	0.22
Pollux	0.00	δ Orionis.....	0.12	β Tauri	0.04
		ζ ——.....	−0.46	δ Orionis.....	0.07
Mean Rate..	−0.03	Mean Rate..	−0.17	ζ ——.....	−0.20
				Mean Rate..	−0.08

Third Series.

1829.	Attached Therm.	Barom.	Therm. A.	Therm. B.	No. of Coin.	Disapp.	Re-app.	Time of Coin. by the Sidereal Clock.	Arc.	Remarks.
	°	inches.	°	°		m s	m s	h m s	°	
Dec. 23	72.0	30.094	71.9	72.2	1	26 48	27 4	22 26 56	1.29	Closed the clock door; the face open.
	71.0	30.070	70.0	70.3	28	48 33	48 52	0 48 42½	0.58	
24	66.0	30.095	65.0	65.5	1	45 0	45 5	11 45 2½	1.84	The light on the right not good, so opened the clock door. Door shut while swinging.
	70.0	30.094	69.1	69.6	62	5 59	6 10	17 6 4½	0.34	
	71.1	39.094	71.9	72.4	1	10 37	10 52	17 10 44½	1.31	Clock door still left open.
"	74.0	30.072	74.3	75.0	24	10 14	10 33	19 10 23½	0.68	
	74.0	30.072	76.0	76.6	1	16 1	16 17	19 16 9	0.92	There may be a slight mistake in the first arc 0.92; the pencil mark on the slate being dim.
"	74.0	30.055	75.0	75.7	31	53 20	53 32	21 53 26	0.40	
	74.0	30.054	75.4	76.0	1	59 12	59 26	21 59 19	1.38	Clock door not quite closed.
"	73.0	30.080	72.9	73.1	33	46 48	49 4	0 46 56	0.50	
25	74.0	30.196	73.1	73.7	1	46 45	46 57	19 46 51	1.64	
	73.0	30.185	73.0	73.5	59	49 52	50 14	0 50 3	0.48	
26	72.0	30.010	71.5	72.0	1	23 25	23 38	12 23 31½	1.60	Clock door shut.
	77.0	30.060	76.2	76.9	65	58 42	58 59	17 58 50½	0.28	
	79.0	30.061	79.9	79.9	1	9 1	9 19	18 9 10	0.98	Lower shutter of the clock open.
"	82.0	30.020	81.6	81.9	21	53 29	53 45	19 53 37	0.58	
	82.0	30.019	82.0	82.0	1	0 50	1 6	20 0 58	1.16	
"	81.0	30.003	80.4	80.6	32	42 30	42 46	22 42 38	0.52	
	81.0	30.002	80.2	80.4	1	53 4	53 22	22 53 13	1.12	
"	79.0	29.970	78.8	79.3	25	58 15	58 33	0 58 24	0.66	
27	74.0	29.942	74.0	74.0	1	40 47	40 55	12 40 51	1.83	Lower shutter of the clock open during this day.
	79.0	29.965	79.8	80.1	65	15 26	15 36	18 15 31	0.28	
	80.0	29.975	80.0	80.1	1	21 4	21 22	18 21 13	1.28	The weather very favourable.
"	80.0	29.976	80.6	80.8	28	41 54	42 12	20 42 3	0.58	
	81.0	29.976	81.0	81.3	1	47 44	48 0	20 47 52	1.34	
"	80.0	29.930	80.0	80.1	29	13 43	14 1	23 13 52	0.60	
	80.0	29.928	80.0	80.2	1	23 52	24 10	23 24 1	1.13	
"	78.0	29.962	78.0	78.5	19	57 46	58 8	0 57 54	0.68	
Mean	76.107	30.034								

Third Series computed.

1829.	Mean Temp.	Interval.	Uncorrected No. of Vibrations.	Clock's Rate.	Correc. for Arc.	Reduc. to 62° Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours.
	°	h m s					
	71.100	2 21 46	86086.56	−0.01	+1.361	+3.831	86091.74
	67.300	5 21 2	86087.82	+0.14	1.587	2.231	86091.78
	73.400	1 59 39	86085.38	+0.14	1.567	4.799	86091.89
	75.825	2 37 17	86085.72	+0.14	0.675	5.820	86092.35
	74.350	2 47 37	86085.22	+0.14	1.336	5.199	86091.89
	73.325	5 3 12	86085.12	+0.14	1.540	4.768	86091.67
	74.150	5 35 20	86085.36	−0.02	1.167	5.115	86091.62
	80.825	1 44 27	86083.60	−0.02	0.974	7.925	86092.45
	81.250	2 41 40	86082.82	−0.02	1.098	8.104	86092.00
	79.675	2 5 11	86082.88	−0.02	1.241	7.441	86091.54
	76.975	5 34 40	86084.30	−0.02	1.428	6.304	86092.01
	80.375	2 20 50	86082.92	−0.02	1.348	7.736	86091.98
	80.600	2 26 0	86082.72	−0.02	1.464	7.831	86091.99
	79.175	1 33 53	86082.86	−0.02	+1.302	+7.231	86091.37
Mean..	76.301					Mean	86091.85
						Buoyancy	+5.85
(No. of Coincidences = 507)					Number of Vibrations in vacuo, in 24 Mean Solar Hours..		86097.70

Rate of the Sidereal Clock.

Dec 23rd.		Dec. 24th.		Dec. 25th, 26th, & 27th.	
	s		s		s
By α Ceti	−0.02	By α Ceti	+0.02	By Rigel	+0.09
Aldebaran	−0.01	Aldebaran	0.26	β Tauri	+0.01
Rigel	−0.05	Rigel	0.17	δ Orionis.....	−0.10
β Tauri	−0.01	β Tauri	0.02	ζ ——.....	−0.08
δ Orionis.....	+0.01	δ Orionis.....	0.00		
ζ ——.....	−0.08	ζ ——.....	+0.35	Mean Rate..	−0.02
Mean Rate..	−0.01	Mean Rate..	+0.14		

Fourth Series.

1830.	Attached Therm.	Barom.	Therm. A.	Therm. B.	No. of Coin.	Disapp.	Re-app.	Time of Coin. by the Sidereal Clock.	Arc.	Remarks.
	°	inches.	°	°		m s	m s	h m s	°	
Jan. 22	67.5	30.088	67.8	68.2	1	16 11	16 29	16 16 20	1.40	Shut the clock door, and kept the Northern chase open.
	68.0	30.085	67.2	67.4	11	8 47	9 5	17 8 56	1.03	
	68.0	30.085	67.8	68.1	1	14 4	14 20	17 14 12	1.01	
"	69.0	30.085	68.2	68.6	11	6 47	7 3	18 6 55	0.76	
	69.0	30.085	69.2	69.7	1	12 4	12 20	18 12 12	0.72	
"	70.0	30.087	69.8	70.2	11	4 48	5 4	19 4 56	0.52	
	72.0	30.087	72.0	72.3	1	18 30½	18 36½	19 18 33½	1.91	
"	72.0	30.084	71.5	71.7	11	10 47	11 2	20 10 54½	1.36	
	72.0	30.084	72.6	73.2	1	16 1	16 17	20 16 9	1.34	Closed the clock door, the Northern chase still open.
"	72.0	30.062	71.2	71.5	11	8 31	8 47	21 8 39	0.97	
	72.0	30.062	71.7	71.9	1	13 45½	14 2½	21 13 54	0.93	
"	72.0	30.055	70.5	70.8	11	6 20	6 36	22 6 28	0.71	
	72.5	30.037	71.2	71.5	1	10 21	10 37	23 10 29	1.47	
"	72.5	30.035	70.2	70.4	12	8 1½	8 18½	0 8 10	1.06	
	72.5	30.035	71.2	71.5	1	13 17	13 33	0 13 25	1.02	
"	72.5	30.043	70.0	70.3	11	5 51½	6 8½	1 6 0	0.78	
Mean..	70.844	30.063								

Fourth Series computed.

1830.	Mean Temp.	Interval.	Uncorrected No. of Vibrations.	Clock's Rate.	Correc. for Arc.	Reduc. to 62° Fahr.	Reduced Vib^{ns} in 24 Mean Solar Hours.
	°	h m s					
	67.650	0 52 36	86087.52	−0.93	+2.481	+2.379	86091.45
	68.175	0 52 43	86088.76	0.93	1.275	2.600	86091.70
	69.725	0 52 44	86088.90	0.93	0.624	3.252	86091.85
	71.875	0 52 21	86084.94	0.93	4.340	4.157	86092.51
	72.125	0 52 30	86086.48	0.93	2.168	4.263	86091.98
	71.225	0 52 34	86087.18	0.93	1.095	3.984	86091.33
	70.825	0 57 41	86085.86	0.93	2.600	3.715	86091.24
	70.750	0 52 35	86087.34	−0.93	+2.053	+3.684	86092.15
Mean..	70.294					Mean....	86091.78
						Buoyancy.....	+5.91
(No. of Coincidences = 81)						Number of Vibrations in vacuo, in 24 Mean Solar Hours... ..	86097.69

Rate of the Sidereal Clock.

Jan. 22nd.	s
By Castor.....	−0.90
ε Geminor.....	0.96
Sirius	0.82
δ Geminor.....	0.90
Castor.....	0.80
Procyon	1.02
Pollux.....	−1.08
Mean Rate..	−0.93

Abstract of Mr. Fallows's observations.

Mean of 401	coincidences	. . .	= 86097.67
Mean of 419	coincidences	. . .	= 86097.66
Mean of 507	coincidences	. . .	= 86097.70
Mean of 81	coincidences	. . .	= 86097.69
True Mean of 1408	coincidences	. . .	= 86097.68

Note by Captain Sabine.

The observations which Mr. Fallows has communicated to the Society in this memoir, having been corrected for buoyancy and expansion, before the volume of the Phil. Trans. had reached the Cape, in which the true value of those corrections is assigned from experiments with a pendulum of the same materials and figure as that employed by Mr. Fallows, I have re-computed his results with the correct elements of reduction, and find the retardation of the vibrations at the Cape, compared with those in London, to be 67.15 per diem, instead of 67.12, the difference between Mr. Fallows's calculation and mine amounting only to three hundredths of a vibration per diem.

The small amount of the difference, on the employment of the more correct elements of reduction, is an illustration of the remark, with which I concluded the paper on the reduction to a vacuum of an invariable pendulum (Phil. Trans. 1829, page 236.); that in relative experiments, computed before the true reduction to a vacuum was known, and in which a correction for expansion was employed, derived directly from the vibration of the pendulum at the same spot in different temperatures, (as is the case in Mr. Fallows's calculation,) a compensation takes place of the errors of the respective reductions for expansion and resistance, leaving the only uncompensated error in the final result, that arising from barometric differences, which in all cases of comparison between stations not far removed from the level of the sea, cannot be otherwise than extremely small.

In Mr. Fallows's calculation he has taken the rate in London of the invariable pendulum which Captain Ronald took out to the Cape, solely from my observations with it: if, however, Captain Ronald's observations with the same pendulum in London be added to mine, and a true mean be taken corresponding to the number of observations of each observer, the retardation is precisely that stated by Mr. Fallows; namely, 67.12 vibrations per diem.

XII.—*Statement of the principal circumstances respecting the united Siamese Twins now exhibiting in London. By* George Buckley Bolton, *Esq., Member of the Royal College of Surgeons, and of the Medical and Chirurgical Society of London. Communicated by the President.*

Read April 1, 1830.

THE youths who are the subject of the following memoir, were born in May 1811 in the kingdom of Siam, at Maklong, a small village sixty miles distant from the capital, Bankok. They are the offspring of Chinese parents, and have been named, the one Chang, and the other Eng. Bankok is situated on the river Minam, forty miles from its mouth, between the Burmese and Chinese empires, in latitude 13° north, and longitude 101° east. Siam is tributary to the latter empire.

The king of Siam soon heard of the birth of Chang and Eng, and at first designed to have them put to death, conceiving them to be monsters, and imagining that the existence of such beings portended some evil to his kingdom. But afterwards hearing that they were harmless, and would probably be able to support themselves by labour, he allowed them to remain unmolested. Mr. Robert Hunter, a British merchant resident at Siam six years ago, saw the twins for the first time in a fishing-boat on the river Minam. They were naked from the hips upwards, were very thin in their persons, and it being then dusk, he mistook them for some strange animal. Shortly afterwards he endeavoured to prevail on the Siamese government to allow them to visit England, but could not at that time succeed. However, in March 1829, Captain Coffin and Mr. Hunter conjointly obtained this permission for a certain period, having gained the ready acquiescence of their mother, on the condition of a provision being made for her during the absence of her sons.

The mother is stated by Captain Coffin to be about five feet seven inches in height, well formed, with large hips, and, for her country, a strong woman,

though of lax fibre. She was thirty-five years of age when her twins were born, and had previously given birth to several other children, none of whom had any malformation. She declared to Captain Coffin that she suffered less during her pregnancy with these than on any similar occasion, and also that her labour was not attended with the least difficulty. She further stated that the twins were born with the head of one between the legs of the other, and were rather small infants. All her other children, except these and two others, a brother and sister, are dead.

The united twins left Siam in the beginning of April 1829, with Captain Coffin and Mr. Hunter, on board the American ship Sachem, and arrived at Boston in the United States in the following August, where they remained eight weeks, during which period they excited the greatest interest among scientific and professional men.

They embarked at New York for England in the October following, and arrived in London on the evening of the 19th of November 1829. On the 24th of November an invitation was given to the most distinguished persons of the medical faculty in London, to view them at the Egyptian Hall, Piccadilly, and on the 26th of November I was desired to become their sole medical attendant.

These youths are both of the same height, namely, five feet two inches; and their united weight is one hundred and eighty pounds. They are much shorter, and appear less advanced in puberty, than youths of this country at the age of eighteen years; but the average stature of their countrymen is less than that of Europeans. Many of our ordinary twins bear a stronger resemblance to each other in countenance than is observed in these youths. Their bodies and limbs are well formed, but the spine of Chang, who habitually holds his arm over the shoulder of Eng, is considerably curved laterally, an effect which is apparently the result of this long continued habit. They have not the broad and flat forehead so characteristic of the Chinese race, but resemble the lower class of the people of Canton in the colour of their skins, and in the forms of their noses, lips, eyes, and ears. The left eye of Chang is weaker than the right; but this is reversed in the case of Eng, so that each sees best with the eye nearest his brother. Their bodies are much paler now than they were on their first arrival in England. Their genital organs are,

like all their other external parts, regularly formed; but the youths are naturally modest, and evince a strong repugnance to any close investigation on this subject.

The band of union is formed in the following manner.—At the lowest part of the sternum of each boy, the ensiform cartilage is bent upwards and forwards, meeting the other in the middle of the upper part of the band, where moveable joints exist, which admit of vertical as well as lateral motion; each junction appearing to be connected by ligamentous structures. It is difficult to define precisely where the respective cartilages from each body meet, and whether a slip from one of the cartilages of the false ribs enters into the structure of these parts; but it is certain that the ensiform cartilages have assumed an extended and altered figure. This cartilaginous portion occupies the upper region of the band. The outline of the band is convex above, and arched below. Under the cartilage, while they stand in their ordinary posture, are large hernial sacs opening into each abdomen, and into which, on coughing, congenital herniæ are forced; probably, in each boy formed by a portion of the transverse arch of the colon: generally, however, and under ordinary circumstances, these herniæ are not apparent. Whether there is a communication between the two abdominal cavities, or a distinct peritoneal sac belonging to each hernia, is by no means obvious; and this is a point of vital importance, if ever, by their mutual desire, a surgical separation should be contemplated. If, however, any such operation hereafter be strongly requested by both the youths, when arrived at years of discretion, and after they have been fully apprised of its danger, it will be essential that some preliminary steps be taken to provide against the exposure of either or both of the abdominal cavities.

When these herniæ protrude, their respective contents are pushed forwards as far as the middle of the band. The entire band is covered with common integument; and when the boys face each other, its length at the upper edge is one inch and three quarters, and at the lower, not quite three inches. From above downwards, it is three inches and a quarter, and its greatest thickness is one inch and five-eighths. In the centre of the lower part of this band, which presents a thin edge, formed only by skin and cellular substance, there is the cicatrix of a single navel, showing where the umbilical cords or cord had

entered, and which I have no doubt contained two sets of vessels*. Small blood-vessels and nerves must of course traverse the substance of the band, but no pulsation can be detected in it.

Captain Coffin and Mr. Hunter were informed by the mother of the twins, that soon after their birth, and during the period of infancy, this band was much larger in proportion to the size of their bodies than it is at the present time: it had then no hard cartilaginous feel at its upper margin; it was also larger in circumference, and the bodies of the twins were nearer in contact; but from continued stretching it has become elongated, and its circumference has diminished. In their own country they were employed to row a boat, for which purpose both stood at the stern, each using a one-handed oar, an exercise which must have assisted greatly in stretching the band. It is now remarkably strong, and possesses little sensibility; for they have been formerly pulled by a rope fastened to it, without complaining of pain, or expressing any uneasiness. In the month of February last one of them fell out of bed while asleep, and hung by the band for some time, and when both awoke, they alike stated, that they experienced no pain in the band from this accident. Mr. Hale, their constant attendant, has lifted one of them from the ground, allowing the other to hang by the band with his feet raised from the floor; yet the whole weight of one of the boys thus suspended did not occasion pain to either, or even excite their displeasure. The circumstance of the small degree of sensibility possessed by the band, tends to corroborate the opinion I entertain of the possibility of effecting a separation of the twins by a surgical operation.

* It has been asserted, that "these twins are the produce of a single ovum, and grew upon one placenta, by one umbilical cord;" but of this there does not appear to be any evidence. By permission of the Board of Curators, I have had an opportunity of examining a preparation of united female twins, now in the museum of the Royal College of Surgeons in London. The union extends from the lower part of the sternum of each twin to the navel; and there is one umbilical cord common to both. On dissection, the following appearances were observed.—The umbilical vein in its course towards the twins, is divided into two nearly equal sized branches, the division taking place at about one inch and three quarters from the umbilicus; one branch passing upwards in front to the porta of the anterior liver, and the other behind to its proper liver. The number of arteries are four, two from each fœtus, and are included in the same theca with the umbilical vein as far as the body, retaining the appearance of an ordinary funis.

To the ordinary touch there is not any middle line where the sense of feeling common to both the boys terminates; but it is difficult to ascertain the precise point where the inosculation of the one individual with the other takes place: and this is not discoverable by making punctures with a needle, for each boy shrinks from a puncture whenever it is made in any part of a vertical line drawn down the middle of the band. It is therefore obvious, that the nerves of the common skin covering the band maintain a sensitive communication with each of the two youths; and it is reasonable to infer, that a similar communication subsists between the small arteries and veins, which mutually nourish the middle portion of the band. If, however, slight punctures be made at the distance of half an inch from the centre of the band, then the sensation is only felt by the individual belonging to the side punctured.

From these evidences it may be concluded, that the united twins would be subject to certain distempers in common, although each possesses a distinct existence, and even different constitutional peculiarities.

On the suggestion of Doctor Roget, a silver teaspoon was placed on the tongue of one of the twins, and a disk of zinc on the tongue of his brother: when the metals thus placed were brought into contact, the youths both cried out "Sour, sour." This experiment was repeated several times with the same result, and was reversed by exchanging the positions of the metals, when a similar effect was produced.

These experiments prove that the galvanic influence passes from one individual to the other, through the band which connects their bodies, and thus establishes a galvanic circuit with the metals when these are brought into contact.

They habitually face in one direction, and place themselves side by side, Eng to the right and Chang on the left, but are able to turn and remain in the opposite position. They always walk in the posture first described, although there is no other reason for this than established habit, as they are physically able to move in a reverse direction. Their united strength is great, for they can with perfect ease throw down a powerful man. At Philadelphia they also carried without inconvenience a person weighing rather more than twenty stone for a considerable distance. Their activity is remarkable; they run with great swiftness, and elude pursuit so admirably, that in sportive exercises they can

with great difficulty be caught by a single person. They have each the power to bend their bodies in all directions, and turn their heels over their shoulders. They also often playfully tumble head over heels while on their bed, without occasioning the slightest pain or inconvenience in the band. The same degree of personal dexterity is evinced by each youth when playing at battledore and shuttlecock; and in all the bodily actions common to both, such as running and jumping, a remarkable consent or agreement is displayed without any apparent conference. These concurrences appear to be the necessary result of long continued and extraordinary intimacy.

In their respective physical constitutions, however, several differences occur. The boy on the left, CHANG, possesses the more vigorous bodily health of the two; but their intellectual abilities appear equal, for they are alike proficients in the games of chess and draughts, although they object to compete with each other. In the game of whist, however, they rather prefer not to be partners.

The tongue of ENG is at all times whiter than that of CHANG, and his digestion is more easily deranged by unsuitable diet. I have never heard that CHANG has passed a single day without alimentary discharges, but the contrary has often occurred to ENG. In general they both obey the calls of nature at the same time, and this happens even when these result from the operation of medicines.

It having occurred to me, that the odour given by asparagus to the urine would be a test of the extent of the circulation of the blood through both the twins, on the 22nd of March I gave that vegetable to CHANG with his dinner, not allowing any to be given to his brother. On examining their urine four hours after this meal, that of CHANG had distinctly the peculiar asparagus smell, but the urine of his brother was not influenced by it. The next day this experiment was reversed, and therefore with reversed results. These trials sufficiently prove a fact which was otherwise apparent,—that the sanguineous communication between the united twins is very limited.

On the 9th of December they were both attacked with bronchial catarrhs, became pale and languid, coughed severely, and complained of pain in their throats; each of them had also slight pains during strong inspirations. Their skins were dry and cold, respirations hurried, pulses ninety beats in a minute, rather hard and small; the tongue of ENG was glazed and pallid as

usual, CHANG's became furred and dry. The bowels of both had been naturally relieved the day previous, and each was directed to take such medicines as experience had shown to be proper in the malady now common to both. Under this discipline and suitable diet, together with the additional clothing of leather waistcoats, and a leather coverlet for their bed, then considered to be required on account of the inclement winter, they both regained their ordinary state of health.

These incidents are recorded merely to show that they have been treated as two distinct persons, although from the very unusual circumstance of their conjunction, the same causes of disorder are presented to both, and similar consequences have thence ensued.

They have had the measles, and at eight years of age the confluent small-pox, distinct marks of the latter disease being still visible on their faces. When they were attacked with variola, a brother and two sisters had also that malady so severely as to occasion their deaths. While lying on their backs in bed, during their late illness, I counted the pulse of ENG in this position; it was sixty-three, soft and regular. I then went round the bed to examine CHANG, but in the mean time both had had occasion to move, and returned immediately to their former position. Although these movements were effected quietly, and did not occupy half a minute, producing no coughing, yet on counting CHANG's pulse, it was eighty, and soft. I then again counted ENG's, which was also eighty, soft and regular. After a lapse of ten minutes, both having remained perfectly still, on counting their pulses I found them at seventy-two.

I have submitted these occurrences in order to show the reciprocity of their symptoms under similar conditions of disorder.

In a healthy state, their ordinary pulses are generally alike, but CHANG's is the strongest; they are both easily excited, and when in one the pulse has been, from this cause, raised to ninety, that of the more passive brother has remained at seventy-two. They always take their meals together, objecting to be seen while thus engaged. Neither will eat or drink what the other dislikes, though they occasionally take different sorts of food at the same time, such as meat or fish. When the appetite of the one is satiated, the other is also satisfied. In their habits they are very cleanly and delicate, and mutually assist each other in

dressing. They are exceedingly affectionate and docile, and grateful for every kindness shown them. It is not often that they converse with each other, although their dispositions and tempers agree, and their tastes and opinions are similar. Sometimes they engage in distinct conversations with different persons at the same time, upon totally dissimilar subjects. Both are very fond of music, and are equally interested in dramatic performances.

It does not appear that they have ever had any serious quarrel, except on one occasion, which occurred, as their mother reported, when they were eight years of age. While on their passage to America, one of them wished to bathe, as was their custom, to which the other objected, the day being cold; a slight dissension ensued, but Captain COFFIN soon reconciled the difference.

They always fall asleep at the same moment, and it is impossible to wake one without also arousing the other. When they were at Boston, Doctor SKEY, Surgeon General to the British Army, entered their bed-room at midnight on three successive nights when both were asleep: on each occasion he touched one and was answered by the other, both awaking at the same instant, inquiring why they were disturbed.

The experiment has also been repeated in this country, and with the same result. On my tickling one of them, the other told me to desist, though he stated that he did not feel the touch, and it was quite clear that he could not see me tickle his brother.

On their voyage to England one of them had the tooth-ache, during three days and nights, and suffered great pain, with loss of sleep, which last evil was shared by his brother, both remaining awake. On the 16th of December Mr. HALE went into their bed-room when they were asleep. ENG was restless, and tossing about in bed, while CHANG was screaming. He awoke them, and on inquiring what ailed them, ENG replied that he was dreaming about his mother, and CHANG said that a man was cutting off the long hair from his head. These different dreams appear to have occurred simultaneously.

The preceding instances of their mutual consent in many physical and moral particulars, may be accounted for by their constant moral and physical intimacy, which unquestionably is the source of more impressions common to both, than ever happen to two distinct individuals. They are at present very much attached to each other, but judging from what is now become a very

common subject of discourse between them, it is not an unreasonable conjecture, that some female attachment, at a future period, may occur to destroy their harmony, and induce a mutual and paramount wish to be separated.

They are remarkably quick in intellect, and possess great imitative powers. They also observe very minutely whatever is presented to them, and comment upon the subject to their friends. On the 29th of November, a gentleman visited them with me, and recommended Captain Coffin to have them taught their letters, and to write. By way of experiment he marked with a pencil, on a card, a large A; he then pronounced the letter, which sound the boys exactly imitated. He afterwards formed a B and a C; but while doing this, Chang interrupted him, wishing to obtain the pencil; and both not only repeated the sounds of the three letters, but imitated their forms, Chang even making a pun on the letter C; for on being asked if he knew its form and pronunciation, he replied, laughing, "Yes, I *see* you."

A person who had lost an eye, visiting them at their exhibition-room in New York, they inquired of their attendant what he had paid for his admission; and on being informed that it was the same as other persons, they remarked that half of it should be returned, as he had not had the same advantage as the others.

These extraordinary individuals are the most remarkable instances on record of perfect and distinctly formed human beings united together, who have attained the age of puberty in a state of sound bodily health: hence an authentic account of their moral and physical habitudes will probably be deemed valuable*.

On concluding this report, I wish it to be known, that I have neither instituted, nor permitted to be made, any unjustifiable experiments upon these youths; considering myself bound, by professional responsibility, as well as by a sense of national justice, to resist all such improper proposals. For these reasons, and also because I am averse to the administration of mercury, unless it be imperatively demanded, I have not had an opportunity of knowing whether the mercurial influence would pervade the one youth, if applied

* There is a case of a double female monster, born at Szony in Hungary, October 26th, 1701, who died February 23rd, 1723, at Presburg, in the convent of the Nuns of St. Ursula, recorded in the Philosophical Transactions for 1757, page 311, by Justus Joannes Torkos, M.D. F.R.S.

exclusively to the other. As, however, the capillary blood-vessels of each unquestionably inosculate with those of his companion in the uniting band, it must be obvious, that certain constitutional diseases, and many diffusible medicinal substances, would, partially at least, pervade both the united twins, though only one of them were exposed to their influences.

In addressing these particulars and observations to the Royal Society, I have intentionally confined myself to a narration of facts, and have abstained from discussions of a speculative or hypothetical character.

I cannot here deny myself the pleasure of stating the kindness which has at all times been evinced towards these youths by Captain Coffin, Mr. Hunter, and Mr. Hale: the unwearied anxiety manifested by these gentlemen for their welfare and happiness, and the liberal manner in which they have uniformly afforded the means of investigating so curious an object of philosophical inquiry, entitle them equally to the thanks of the philanthropist and the lover of science.

3 *King Street, St. James's Square,*
March 29th, 1830.

The Plate exhibits two reduced views of the uniting band; also a representation of a section of the fac simile in wax.

Fig. 1. represents the front view of the ordinary standing attitude of the youths.

Fig. 2. exhibits the opposite side; or what may be termed the ordinary back view.

Fig. 3. represents a section through the middle of the band in its relaxed or unstretched state.

XIII. *On some properties in achromatic object-glasses applicable to the improvement of the microscope.* By JOSEPH JACKSON LISTER, *Esq.* *Communicated by Dr.* ROGET, *Secretary.*

Read January 21, 1830.

THE improvement of the achromatic compound miscroscope having been an occasional object of my leisure for several years past, my attention has in consequence been attracted to some properties of object-glasses of short focus and large aperture, which, so far as I am aware, have not been before noticed, and which, I flatter myself, may be applied to increase its powers and the ease of its manufacture.

In offering these, accompanied by some other miscellaneous remarks, it may be explanatory to introduce them by a short notice of the several achromatic object-glasses for the microscope which have originated apparently independent of each other within a few years past.

The first produced in England were the triple ones constructed in 1824 and 1825 by TULLEY, who had been incited to the undertaking by Dr. GORING: of these an account is already before the public. TULLEY has since adopted for his triple object-glass to be used singly the focal length of 0.9 inch, applying another of not quite 0.5 inch focus before it when high magnifying power is required; and he obtains with these an image of great sharpness and perfection. His first glasses have all the merit of a new invention, having been executed without the knowledge that any thing of the kind previously existed, though other achromatic glasses had before been made in France by SELLIGUE, by FRAUNHOFER at Munich, and by AMICI at Modena.

The glasses of SELLIGUE'S microscope, of which a report was made by M. FRESNEL to the Royal Academy of Sciences in 1824, were composed of a plano-concave lens of flint-glass, and a double convex of crown or plate, with their inner curves cemented together. Four of these, of from $1\frac{1}{2}$ to $1\frac{7}{8}$ inch

each in focal length, were made to screw before each other, so as to be used together or alone in the manner long practised with single lenses.

The chromatic aberration was thus in a considerable degree corrected, but the glasses were fixed in their cells with the convex side foremost, which is their worst position; and the spherical error was in consequence enormous, showing itself even through the contracted opening, to which it was necessary on that account to limit them.

Yet, inferior as was the instrument of Selligue, the happy idea of combining achromatic object-glasses, now generally adopted and to which their present superiority is owing, seems to have occurred to no one else till put in practice by him; and the very simple structure of his glasses will be shortly seen not to be incompatible with the finest microscopic vision.

Chevalier of Paris having manufactured some of these instruments, appears to have observed the great error in the position of Selligue's glasses; he retained their construction, but turned their plane sides foremost; and making them of shorter focal length and more correctly achromatic, produced in 1825 a microscope far superior to the former. His deepest glasses are not more than 0.4 inch in focal length, and two of these were united in his earlier instruments for his highest power; but this was the only combination retained in them, and all his glasses were restricted to apertures too small to show difficult test objects.

He has since increased the number of his glasses to be used together, and otherwise improved their performance; but if I may judge from microscopes of his which I have recently seen in this country, he does not yet derive from the construction that he has adopted, all the advantage which it may afford.

I am unacquainted with the date of the first production of Fraunhofer's glasses; they resemble the French in having their flint lens plano-concave, but they are not cemented, and the inner surfaces are not in contact, each object-glass being adapted by the curves of its convex lens for being used alone. My friend Mr. Brown has kindly lent me a series of five glasses of this description, purchased by him at Munich a few months ago from the establishment of Utzschneider and Fraunhofer, which range from 1.8 to 0.43 inch in focal length, and are of excellent workmanship: they screw before one another in the manner of Selligue's. It appears from an account of the microscope of

those artists, printed in 1829, that it is only lately the glasses have been combined together, and that of the shortest focus added to the former series of four; and also that they are intended to be used in the order of their focal lengths, the shorter towards the object. Each of these glasses singly admits but a small pencil of from 8° to 15° of light, and when so used, their defining power is necessarily not very great; but their combinations have much more, and the different effects of these will be again adverted to.

The eminent professor of Modena, besides inventing his well-known reflecting microscope, was engaged about the year 1815 with achromatic object-glasses; but as they did not equal his reflector, he laid the work aside, till he was induced to resume it in 1824, from reading the report already mentioned on the microscope of Selligue: from this Amici took the thought of combining his object-glasses, and pursued it with great success, making them double, with the curves of the two lenses of each planned for the place it is to occupy, and for obtaining a good image either with the back glass alone, or with a second or a third in front.

He brought with him, when he visited London in 1827, some glasses of this description of very fine performance; and I have been informed by him that he has since executed a combination of "2.7 lines in focal length and 2.7 lines in aperture," which considerably excels them.

The glasses which have been enumerated possess very different degrees of merit, chiefly dependent on the extent to which they are divested of chromatic and spherical aberration, and particularly, in connection with this, on the focal angle of their aperture; for it has been well established, that a large pencil from the object is absolutely essential to that brilliancy and distinctness of image which characterize a fine achromatic.

When the rays received by the most perfect object-glass from any indefinitely small bright portion of an object in the centre of its field are brought together at its conjugate focus, the image formed by them, though it appears a sharply defined point if moderately magnified, is really a spot or small circle, and will show as such if the microscope is sufficiently overcharged with power in the eye-glass. These circles bear a considerable analogy to the spurious disks of stars; and like them they will be found to be much enlarged by diminishing the aperture of the object-glass.

They are enlarged also, without contracting the aperture of the glass, by

increasing the intensity of the illumination, whereas by darkening the object beyond a certain point they may be rendered ill defined, and be at length dissolved.

These peculiarities are most observable on some opaque objects, (the reflection from a very small microscopic globule of quicksilver* offers perhaps the best example,) but the same effects are produced on the light received from transparent ones; and the consequent blunting and mingling together of their minute details when the object-glass admits but a small pencil of light, gives rise to various fallacious appearances. One of the most remarkable is the spottiness which some surfaces assume, not unfrequently so much resembling small globules as to have been mistaken for them; an optical illusion having thus been the basis of some ingenious speculations on organic matter.

Such appearances have little place with the finer achromatics, the large angle of whose pencil and its accurate correction enable us to magnify their image greatly, still discovering something new in our object, before we are checked by the circles of diffusion of the effective rays; but these at last, whether proceeding from the causes mentioned or from others to be hereafter noticed, form in every microscope the boundary to defining power, except where faulty materials or workmanship give it an earlier limit.

It is the marginal rays which contribute especially to render visible close and delicate lines, such as those on the scales of lepidopterous insects, and some of the most difficult of these are even best seen when the central light is intercepted†.

A glass that is far from correct in its figure will sometimes show lines of this description sharply, while the outline of the scale is indistinct, and the

* To obtain such, I have always placed a globule on a piece of black glass, and scattered it into many by a smart stroke, some of which will be extremely minute.

† The blue down of the Menelaus and the white of the Cabbage Butterfly (Morpho Menelaus and Pieris Brassicæ) well deserve the place they have acquired as standard tests, especially on account of the respective characters of their transverse tracings. Some of the small oval scales from the body of the Twenty-plumed Moth (Alucita hexadactyla) are among the closest and most elegant in their lines that I have seen; others are very easily resolvable, and the down has such diversity of form as often to afford within a short space a ready variety of excellent tests. The same is to be said of the scales of Podura plumbea, of which all are difficult, and some seem to defy all powers of definition.

It may be observed, that with most objects there is such difference between individual specimens of the same kind, that in general the only safe way to determine between two good microscopes, is to apply the same specimen to both under the same circumstances of power and light.

contrary; but one in which both aberrations are destroyed should give the outline and the lines distinct together. Even some good glasses, however, have a defect, against which we should be on our guard; that in certain directions of the light they are liable to show lines on an object which do not really exist.

It is observable, that provided the aperture of the glass remains open, the central pencil of light admitted behind many transparent objects may be limited to a very small one without greatly impairing their sharpness; parallel lines or spots spread closely over a flat surface often remaining plainly visible in this case, which at a far less amount of contraction by a stop behind the object-glass cannot by any management be made to appear. The reason of this seems to be, that both reflection and refraction of a part of the rays take place at such objects, by which the pencil is spread out on leaving them to a much increased angle in its progress to the glass.

The relation between the aperture of microscopic object-glasses, even of the same focal length, and the pencil of light admitted by them, will vary much, according to differences in their thickness, their combination &c.; and as aperture is valuable only in proportion to the pencil it admits, the latter would seem to be the circumstance the more deserving attention of the two. It is so often erroneously estimated, that I will mention a simple mode of ascertaining it, which will be found pretty accurate.

Fix a piece of paper on a table, and on it place the microscope with its body horizontal, and one of the eye-pieces on; set a candle on a level with it a few yards distant; then having directed the body of the instrument so far on one side of the candle, as that the light from it shall bisect the field vertically, leaving half of it dark, trace on the paper a line corresponding to the side of one of the legs. Now, taking the focus of the object-glass as a pivot, turn the microscope horizontally to the other side of the candle till the opposite half of the field only is illuminated, and mark again on the paper the position of the side of the leg. The measure of the angle traversed shown by the two lines is that of the pencil of light.

In the remarks which follow, the term correction is used to imply the effect produced by the denser concave lens of a compound object-glass upon the aberration of its convex. Thus as in a simple convex lens, the rays which pass through it near the circumference have their foci shorter than the more

central rays, and the colours of the violet side of the spectrum into which each ray is refracted have also their foci shorter than those of the red side; if either of these errors is but partially removed by the concave lens, the glass is said to be under-corrected as to that aberration, and over-corrected if the opposite error is produced by it.

A large focal pencil free from all aberration is evidently the great requisite for the object-glass of the compound microscope; a second point desirable to be attained is, that the field should be flat and well defined throughout; and a third, that the light admitted should as much as possible be only such as goes to form the picture, and should not be intercepted or diffused over the field by too many reflections.

The prominent obstacle to obtaining a sufficient pencil for high powers by one object-glass of large aperture and deep curves, is that the correction for the spherical figure by the concave lens is greater for the rays of the circumference than its due proportion to that for the more central ones; so that when such a glass is corrected for the mean of the pencil, if we suppose its disk divided into a central space and three rings surrounding it, the rays which pass through the central space and those of the second ring from it will arrive at their focus when those of the first ring will have just crossed the axis, and those of the marginal ring will not quite have reached it. The injury resulting to the defining power is in similar glasses inversely as the squares of their focal lengths, as far as regards this cause of error, as well as those which arise from incorrigible colour and defects of workmanship. The effects upon the pencil which have been before described, must not be included under the same law. This excess of correction in the marginal rays increases after a certain point so rapidly with a small enlargement of the aperture of the glass, as soon to prescribe a limit, beyond which it cannot be carried without injury to the picture.

With glasses of more contracted aperture and at the several surfaces of which the marginal refraction is moderate, the effect alluded to is comparatively inconsiderable; and consequently by dividing the refraction among two or more such glasses corrected for the rays that pass through them, the pencil received may be enlarged without impediment, and the light and distinctness greatly increased, thus constituting the important advantage of combination.

The triple object-glass is thus very superior to a double one when each is used singly, and the union of two triple ones has been already proved in England to be eminently effective. Tulley's 0.9 inch glass singly admits a pencil of near 20°, and his combination, one of 38°.

These triple glasses and the double ones of Professor Amici are adapted by the form of their curves each to its respective place; but the foreign double glasses which have their flint lens plano-concave, and particularly those of Utzschneider before mentioned, are made much on one model, and intended to be each good alone. It might seem but reasonable to infer from this, that they would be unfit to be combined; and accordingly when screwed together, most of the numerous practicable changes of his series of five glasses, of which as many as four may be united for use, have much indistinctness from spherical error; and this is I think the case with all those combinations which the maker contemplated. Some peculiarities, however, observed two years ago in Chevalier's object-glasses, led me to undertake a close examination of these, which were liberally placed at my disposal for the purpose, in the hope of discovering the cause for a discrepancy which appeared in their effects.

I found that with a part of the combinations, the image of any bright point that was at some distance from the centre of the field had a faint light or coma stretching outwards from it; with others the coma was as much inwards.

The spherical aberration was in general much over-corrected, but in some triple and quadruple combinations the opposite error showed itself; and out of the whole number one triple and one quadruple were remarkably beautiful and distinct.

The result of this investigation was to disclose or confirm to me the existence of some properties of the double object-glass, which have not I believe been hitherto recorded, and which it is now my purpose to describe only in connection with the subject before us,—the improvement of the microscope.

With this in view I would premise that the plano-concave form for the correcting flint lens, which was probably adopted at first for its simplicity, has in that quality a strong recommendation; particularly as it obviates the danger of error which otherwise exists in centering the two curves, and thereby admits of correct workmanship for a shorter focus. To cement together also the two lenses of the glass, diminishes by very nearly half, the loss of light from reflexion,

which is considerable at the numerous surfaces of a combination. I have thought the clearness of the field and brightness of the picture evidently increased by doing this; it prevents any dewiness or vegetation from forming on the inner surfaces; and I see no disadvantage to be anticipated from it, if they are of identical curves and pressed closely together, and the cementing medium permanently homogeneous.

These two conditions then, that the flint lens shall be plano-concave, and that it shall be joined by some cement to the convex, seem desirable to be taken as a basis for the microscopic object-glass, provided they can be reconciled with the destruction of the spherical and chromatic aberrations of a large pencil.

Now in every such glass that has been tried by me, which has had its correcting lens of either Swiss or English flint glass, with a double convex of plate, and has been made achromatic by the form given to the outer curve of the convex, the proportion has been such between the refractive and dispersive powers of its lenses, that its figure has been correct for rays issuing from some point in its axis not far from its principal focus on its plane side, and either tending to a conjugate focus within the tube of a microscope, or emerging nearly parallel.

Let *a b* be supposed such an object-glass, and let it be roughly considered as a plano-convex lens, with a curve *a c b* running through it, at which the spherical and chromatic errors are corrected, which are generated at the two outer surfaces; and let the glass be thus free from aberration for rays *f d e g* issuing from the radiant point *f*; *h e* being a perpendicular to the convex surface, and *i d* to the plane one. Under these circumstances the angle of emergence *g e h* much exceeds that of incidence *f d i*, being probably almost three times as great.

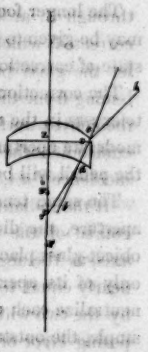
If the radiant is now made to approach the glass so that the course of the ray *f d e g* shall be more divergent from the axis, as the angles of incidence and emergence become more nearly equal to each other, the spherical aberration produced by the two will be found to bear a less proportion to the opposing error of the single correcting curve *a c b*; for such a focus therefore the rays will be over-corrected.

But if f still approaches the glass, the angle of incidence continues to increase with the increasing divergence of the ray, till it will exceed that of emergence which has in the mean while been diminishing, and at length the spherical error produced by them will recover its original proportion to the opposite error of the curve of correction. When f has reached this point f'', (at which the angle of incidence does not exceed that of emergence so much as it had at first come short of it,) the rays again pass the glass free from spherical aberration.

If f be carried from hence towards the glass, or outwards from its original place, the angle of incidence in the former case, or of emergence in the latter, becomes disproportionately effective; and either way the aberration exceeds the correction.

These facts have been established by careful experiment: they accord with every appearance in such combinations of the plano-convex glasses as have come under my notice, and may I believe be extended to this rule;—that in general an achromatic object-glass, of which the inner surfaces are in contact, or nearly so, will have on one side of it two foci in its axis, for the rays proceeding from which it will be truly corrected at a moderate aperture; that for the space between these two points, its spherical aberration will be over-corrected, and beyond them either way under-corrected.

I am not aware that an exception is to be made for any quality of glass or curves that are likely to be used for the microscope: but I apprehend a case may occur, if the flint glass is convexo-concave and the convex lens united to its concave side, that neither of the aplanatic pencils may converge after traversing the glass, and that their foci for a radiant may be on opposite sides of it, the principle however of the two foci remaining unaltered.



To try this principle under a great change of circumstances, and to prove in what manner it was applicable to another most simple form of the object-glass, having made a magnified tracing of the curves of one of Utzschneider's*, and drawn a ray through it from its longer aplanatic focus,

* See Figure p. 194.

which was ascertained after cementing the lenses; I laid down a figure of the flint lens inverted, and by means of the angles of the other diagram projected a plano-convex *x* to be joined to its flat side. The new glass proved as achromatic as the original, though the single radius of the plano-convex was more than one-third longer than corresponded with the curves of the former double convex lens: the two aplanatic foci showed themselves as before; the longer, however, not in the place indicated by the ray drawn: but at a point F, about two-thirds more distant from the glass, and from which both surfaces of the flint lens bent the pencil outwards; the shorter focus F″ too becoming about half the length of the other, and the angle of incidence of its rays not equalling that of their emergence.

The longer aplanatic focus may be found when one of the plano-convex object-glasses is placed in a microscope, by shortening the tube if the glass shows over-correction, if under-correction by lengthening it, or by bringing the rays together should they be parallel or divergent, by a very small good telescope. The shorter focus is got at by sliding the glass before another of sufficient length and large aperture that is finely corrected, and bringing it forwards till it gives the reflexion of a bright point from a globule of quicksilver, sharp and free from mist, when the distance can be taken between the glass and the object.

The longer focus is the place at which to ascertain the utmost aperture that may be given to the glass, and where, in the absence of spherical error, its exact state of correction as to colour is seen most distinctly.

The correction of the chromatic aberration, like that of the spherical, tends to excess in the marginal rays; so that if a glass, which is achromatic with a moderate aperture, has its cell opened wider, the circle of rays thus added to the pencil will be rather over-corrected as to colour.

The same tendency to over-correction is produced, if, without varying the aperture, the divergence of the incident rays is much augmented; as in an object-glass placed in front of another: but, generally, in this position a part only of its aperture comes into use; so that the two properties mentioned neutralize each other, and its chromatic state remains unaltered. If, for example, the outstanding colours were observed at the longer focus to be green and claret, which show that the nearest practicable approach is made to the

union of the spectrum, they usually continue nearly the same for the whole space between the foci, and for some distance beyond them either way.

The places of these two foci and their proportions to each other, depend on a variety of circumstances. In several object-glasses that I have had made for trial,—plano-convex, with their inner surfaces cemented, their diameters the radius of the flint lens, and their colour pretty well corrected,—those composed of dense flint and light plate have had the rays from the longer focus emerging nearly parallel; and this focus has been not quite three times the distance of the shorter from the glass: with English flint the rays have had more convergence, and the shorter focus has borne a rather less proportion to the longer.

If the inner surfaces are not cemented, a striking effect is produced by minute differences in their curves. It may give some idea of this, that in a glass of which almost the whole disk was covered with colour from contact of the lenses, the addition of a film of varnish so thin that this colour was not destroyed by it, caused a sensible change in the spherical correction.

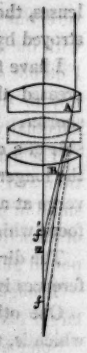
I have found that whatever extended the longer aplanatic focus, and increased the convergence of its rays, diminished the relative length of the shorter. Thus by turning to the concave lens the flatter instead of the deeper side of a convex lens, whose radii were to each other as 31 to 35, the pencil of the longer aplanatic focus, from being greatly divergent, was brought to converge at a very small distance behind the glass; and the length of the shorter focus which had been one-half that of the longer, became but one-sixth of it.

The direction of the aplanatic pencils appears to be scarcely affected by differences in the thickness of glasses, if their state as to colour is the same.

One other property of the double object-glass remains to be mentioned; which is, that when the longer aplanatic focus is used, the marginal rays of a pencil not coincident with the axis of the glass are distorted, so that a coma is thrown outwards; while the contrary effect of a coma directed towards the centre of the field is produced by the rays from the shorter focus. These peculiarities of the coma seem inseparable attendants on the two foci, and are as conspicuous in the achromatic meniscus, as in the plano-convex object-glass.

Of several purposes to which the particulars just given seem applicable, I must at present confine myself to the most obvious one. They furnish the

means of destroying with the utmost ease both aberrations in a large focal pencil, and of thus surmounting what has been hitherto the chief obstacle to the perfection of the microscope. And when it is considered that the curves of its diminutive object-glasses have required to be at least as exactly proportioned as those of a large telescope, to give the image of a bright point equally sharp and colourless, and that any change made to correct one aberration was liable to disturb the other, some idea may be formed of what the amount of that obstacle must have been. It will however be evident, that if any object-glass is but made achromatic, with its lenses truly worked and cemented so that their axes coincide, it may with certainty be connected with another possessing the same requisites and of suitable focus, so that the combination shall be free from spherical error also in the centre of its field. For this the rays have only to be received by the front glass B, from its shorter aplanatic focus f, and transmitted in the direction of the longer correct pencil f A of the other glass A. It is desirable that the latter pencil should neither converge to a very short focus, nor be more than very slightly, if at all, divergent; and a little attention at first to the kind of glass used will keep it within this range, the denser flint being suited to the glasses of shorter focus and larger angle of aperture.



The adjustment for the microscope is then perfected, if necessary, by slightly varying the distance between the object-glasses; and after that is done, the length of the tube which carries the eye-pieces may be altered greatly without disturbing the correction; opposite errors which balance each other being produced by the change.

If the two glasses, which in the diagram are drawn as at some distance apart, are brought nearer together, (if the place of A for instance, is carried to the dotted figure,) the rays transmitted by B in the direction of the longer aplanatic pencil of A, will plainly be derived from some point (z) more distant than f'', and lying between the aplanatic foci of B; therefore (according to what has been stated) this glass, and consequently the combination, will then be spherically over-corrected. If on the other hand the distance between A and B is increased, the opposite effects are of course produced.

In combining several glasses together, it is often convenient to transmit an

under-corrected pencil from the front glass, and to counteract its error by over-correction in the middle one.

Slight errors in colour may in the same manner be destroyed by opposite ones; and on the principles described, we not only acquire fine correction for the central ray, but by the opposite effects at the two foci on the transverse pencil, all coma can be destroyed, and the whole field rendered beautifully flat and distinct.

The occurrence of two good combinations among the numerous ones of Utzschneider's glasses will now appear only what might be expected; and more are to be obtained from them by varying the distance of the glasses from each other.

I have very lately seen one of Chevalier's combinations of three glasses, each about 0.4 inch in focus, inferior indeed to these, and of contracted aperture, but which I ought to notice because its image is pretty well corrected with the glasses in the order given to them by the maker. At the same time, I do not suppose him to be acquainted with the principle which constitutes the key to the effect; otherwise he would hardly have failed to apply it more effectually.

The achromatic meniscus that has been described enters well into combination, and may perhaps be useful as the front glass of three, but its power is small in proportion to its curves. It admits too of being cemented at the back of an achromatic plano-convex; and they may make together a powerful compound glass to go before another.

Though the plano-concave form has been proposed for the flint lens, in the larger glasses it might perhaps occasionally be relinquished with benefit.

Some attention has yet to be given to obtain the best effect from combination; so that with the largest pencil that may be found desirable, and sufficient clear space before the front of the glass, the field may be aplanatic, and the focus short. Already, however, the three first plano-convex glasses that have been made for me by Tulley, only for preliminary experiments, the shortest of them of 0.7 inch focus, have produced at an aperture of 50° the most distinct microscopic vision that I have yet met with; and I anticipate no serious impediment to the carrying defining power much further.

These statements are intended only as a notice, for practical purposes and

wholly detached from theory, of facts that have been in part very recently ascertained. In investigating them I have depended chiefly on magnified measurements and diagrams, which, though not strictly correct, may perhaps be as well adapted as other far more exact but difficult methods, for constructing the minute and complex object-glass of a microscope. It is to this that the observations made particularly apply; and should they bring more within reach than previously, the requisites which have been enumerated, I trust they may not be unacceptable to the enlightened optician; especially if in this department he unite, like TULLEY, the zeal of the amateur with the skill of the superior artist.

I intend soon to put to the test of experiment, whether or not the principle of the two aplanatic pencils may be applied to telescopes, in cases in which it is requisite to restrict their length, so as to enlarge their aperture with a corresponding increase of light and distinctness. In the mean time, it would give me pleasure to see that principle demonstrated, if it deserve it, by some abler hand than mine, and treated in a more rigorous manner than my own limited acquaintance with mathematical science qualifies me to undertake.

XIV. *On the pendulum.* *By* J. W. Lubbock, *Esq. F.R.S.*

Read March 11, 1830.

Captain Kater was the first who made use of Huygens's theorem with respect to the convertibility of the centres of suspension and oscillation to eliminate the moment of inertia, and to obtain the length of the simple pendulum by measuring the distance between the knife edges or axes of suspension. But this very ingenious method of determining the length of the simple pendulum must be considered as a first approximation, which is true only when many circumstances which might affect the truth of the result are not taken into account, but of which the following investigation shows that when the experiments are conducted with care, the effect is insensible. It is, however, desirable to ascertain carefully the limits of the errors which may rise from the circumstances to which I have alluded, and to render the theory of Captain Kater's pendulum as perfect as the method of observation. Laplace has given a complete theory of the apparatus used by Borda in the Connaissance des Temps; and he has shown that in the apparatus of Captain Kater, the distance between the knife edges is equal to the length of the simple pendulum, when they are considered as cylinders of small curvature, provided their radii of curvature are equal; which theorem is also proved in Professor Whewell's Dynamics. But no one I believe has yet discussed all the circumstances which affect the accuracy of Captain Kater's method; and I have therefore attempted to do this in the following paper, in which I have treated the question with the utmost generality, taking the case of all possible deviations and of axes any how placed, provided only that they are synchronous.

I have taken the pendulum used by Mr. Baily, and described by him in the Philosophical Magazine of last February, to afford a numerical example, and I

have given the errors which would arise in the length of the simple pendulum corresponding to given deviations of the knife edges: it is difficult to make the results intelligible without the use of symbols; but I may add, that the effect of a small deviation of one of the knife edges in azimuth is quite insensible: this is not the case with a deviation in altitude: a deviation of a degree in altitude increases by 3 the vibrations in twenty-four hours: a deviation from horizontality in the agate planes has a more sensible influence than either of the former deviations: a deviation in horizontality in the agate planes of 10′ increases by about 6 the vibrations in twenty-four hours: both these deviations have the effect of rendering the distance between the knife edges greater than the true length of the simple pendulum. I have also considered the case in which the agate planes are fixed on the pendulum and vibrate on a fixed knife edge; and I find, as might be expected, that the length of the simple pendulum is equal to the distance between the planes.

Let Ox, Oy, Oz be rectangular coordinate axes meeting in the point O in the plane $(x\,y)$, upon which the pendulum rests; let the plane $(x\,z)$ be vertical and the plane $(x\,y)$ nearly horizontal, and let the axis of rotation coincide with the line Ox. Let g be the force of gravity, ϵ the angle which a vertical line makes with the axis Oz, a the distance of the centre of gravity from the line Ox, and $M(a^2 + k^2)$ the moment of inertia of the pendulum about the axis Ox; then, according to the analysis of M. Poisson, (Traité de Mécanique, p. 116.) the length of the simple pendulum which oscillates in the same time is $\frac{a^2 + k^2}{a \cos \epsilon}$.

Let $G\,x_1$, $G\,y_1$, $G\,z_1$ be the three principal axes which intersect each other in the point G, and let the equations to the axis Ox referred to the coordinate axes $G\,x_1$, $G\,y_1$, $G\,z_1$, which are fixed in the pendulum and move with it, be

$$y_1 = x_1 \tan \delta + \beta$$
$$z_1 = x_1 \frac{\tan \delta'}{\cos \delta} + \gamma.$$

δ and δ' are small angles which may be considered as the deviations of the knife edge in azimuth and altitude.

If $a\,y = b\,x + \beta$
$a\,z = c\,x + \gamma$

are the equations to any straight line (ϱ) in space, the equations to a straight line perpendicular to this line, and passing through the origin, are

$$\left.\begin{aligned} &a\,x + b\,y + c\,z = 0 \\ &\gamma\,(a\,y - b\,x) = \beta\,(a\,z - c\,x) \end{aligned}\right\}$$

and the shortest distance from the origin to the given line

$$= \sqrt{\frac{(\beta\,b - \gamma\,c)^2 + \beta^2 c^2 + \gamma^2 a^2}{a^2 (a^2 + b^2 + c^2)}}$$

The equation to a plane passing through the origin and the given line is

$$\gamma\,(a\,y - b\,x) = \beta\,(a\,z - c\,a)$$

and the equations to the intersection of this plane with the plane ($z\,y$) are

$$\gamma\,y = \beta^2$$
$$x = 0$$

If $a'\,y = b'\,x + \beta'$
$a'\,z = c'\,x + \gamma'$

be the equations to any other straight line (ϱ') in space

$$\cos \varrho\,\varrho' = \frac{a\,a' + b\,b' + c\,c'}{\sqrt{a^2 + b^2 + c^2}\,\sqrt{a'^2 + b'^2 + c'^2}}$$

Hence the cosine of the angle formed by the line ϱ and the intersection of the plane

$$\gamma\,(a\,y - b\,x) = \beta\,(a\,z - c\,x)$$

with the plane $z\,y$

$$= \frac{\beta\,b + \gamma\,c}{\sqrt{a^2 + b^2 + c^2}\,\sqrt{\beta^2 + \gamma^2}}$$

the sine of the same angle

$$= \sqrt{\frac{(\beta\,b - \gamma\,c)^2 + \beta^2 c^2 + \gamma^2 a^2}{\sqrt{a^2 + b^2 + c^2}\,\sqrt{\beta^2 + \gamma^2}}}$$

These equations being premised; let C be the point in the axis or knife edge, O x, where a perpendicular let fall upon it from the centre of gravity G cuts it; let C′ be the point where the plane ($z\,y$) cuts the axis O x; and let C″ be the point where one of the surfaces of the pendulum, supposed a parallelepiped, cuts the same knife edge; and let G″ be the point in this surface where a perpendicular let fall from G cuts it.

If half the thickness of the pendulum be called t

$$\mathrm{G\,C} = \mathrm{G''\,C''} \sin \mathrm{C\,C'\,G} - t \cos \mathrm{C\,C'\,G}$$

$$\text{sine C C' G}^2 = \frac{\{\beta \sin\delta \cos\delta' - \gamma \sin\delta'\}^2 + \beta^2 \sin^2\delta'^2 + \gamma^2 \cos^2\delta^2 \cos^2\delta'^2}{\beta^2 + \gamma^2}$$

$$\cos \text{C C' G} = \frac{\beta \sin\delta \cos\delta' + \gamma \sin\delta'}{\sqrt{\beta^2 + \gamma^2}}, \quad \text{if G C'} = a'$$

$\beta = a' \sin\lambda$, $\gamma = a' \cos\lambda$, λ being a small angle

$$\sin \text{C C' G}^2 = \{\sin\lambda \sin\delta \cos\delta' - \cos\lambda \sin\delta'\}^2 + \sin\lambda^2 \sin^2\delta'^2 + \cos^2\delta^2 \cos^2\delta'^2$$

$$\cos \text{C C' G} = \sin\lambda \sin\delta \cos\delta' + \cos\lambda \sin\delta'$$

neglecting $\sin\lambda \sin\delta$ and $\sin\lambda^2 \sin\delta'$

$$\cos \text{C C' G} = \sin\delta', \quad \sin \text{C C' G} = \cos\delta'$$

$$\text{G C} = \text{G''C''} \cos\delta' - t \sin\delta'$$

Let $\varepsilon, \varepsilon', \varepsilon''$ be the angles which the line Ox makes with the coordinate axes G x_ρ, G y_ρ, G z_ρ; and A, B, C the moments of inertia of the pendulum about these axes: then by a well known theorem, if G C $= a$ the length of the simple pendulum

$$= \frac{M a^2 + A \cos\varepsilon^2 + B \cos\varepsilon'^2 + C \cos\varepsilon''^2}{M a \cos\varepsilon_1}$$

$$\cos\varepsilon = \cos\delta' \cos\delta, \quad \cos\varepsilon' = \cos\delta' \sin\delta, \quad \cos\varepsilon'' = \sin\delta'$$

δ and δ' may be considered as the deviations of the knife edge in azimuth and altitude.

C being the point in axis Ox where a perpendicular from G cuts it, the index at foot indicates the knife edge, the length of the simple pendulum if $\varepsilon_1 = 0$

$$= \text{G C}_1 + \frac{A \cos\varepsilon_1^2 + B \cos\varepsilon'^2_1 + C \cos\varepsilon''^2_1}{M \text{G C}_1}$$

let $A = M k^2$, $B = M k'^2$, $C = M k''^2$, if the knife edges (1) and (2) are isochronous

$$\text{G C}_1 + \frac{k^2}{\text{G C}_1} - \frac{k^2 \sin\varepsilon_1^2 - k'^2 \cos\varepsilon'^2_1 - k''^2 \cos\varepsilon''^2_1}{\text{G C}_1}$$

$$= \text{G C}_2 + \frac{k^2}{\text{G C}_2} - \frac{k^2 \sin\varepsilon_2^2 - k'^2 \cos\varepsilon'^2_2 - k''^2 \cos\varepsilon''^2_2}{\text{G C}_2}$$

whence

$$k^2 = \text{G C}_1 \times \text{G C}_2 + \frac{\text{G C}_2}{\text{G C}_2 - \text{G C}_1} \{k^2 \sin\varepsilon_1^2 - k'^2 \cos\varepsilon_1'^2 - k''^2 \cos\varepsilon''^2_1\}$$

$$- \frac{\text{G C}_1}{\text{G C}_2 - \text{G C}_1} \{k^2 \sin\varepsilon_2^2 - k'^2 \cos\varepsilon_2'^2 - k''^2 \cos\varepsilon''^2_2\}$$

The length of the simple pendulum is

$$G\,C_1 + G\,C_2 + \frac{k^2(\sin\varepsilon_1^2 - \sin\varepsilon_2^2) - k'^2(\cos\varepsilon'_1{}^2 - \cos\varepsilon'_2{}^2) - k''^2(\cos\varepsilon''_1{}^2 - \cos\varepsilon''_2{}^2)}{G\,C_2 - G\,C_1}$$

$$G\,C = G''\,C''\cos\vartheta' - t\sin\vartheta'$$

$$= G''\,C''\left\{1 - 2\sin\frac{\vartheta'^2}{2}\right\} - t\sin\vartheta'$$

The apparent length of the simple pendulum $= C''_1\,C''_2$

The true length of the simple pendulum is

$$G''\,C''_1 + G''\,C''_2 - 2\,G''\,C''_1\sin\frac{\vartheta'_1{}^2}{2} - 2\,G''\,C''_2\sin\frac{\vartheta'_2{}^2}{2} - t\sin\vartheta'_1 - t\sin\vartheta'_2$$

$$+ \frac{k^2(\sin\varepsilon_1^2 - \sin\varepsilon_2^2) - k'^2(\cos\varepsilon'_1{}^2 - \cos\varepsilon'_2{}^2) - k''^2(\cos\varepsilon''_1{}^2 - \cos\varepsilon''_2{}^2)}{G\,C_2 - G\,C_1}$$

The angle $C''_1\,G''\,C''_2 = \lambda_1 - \lambda_2$

$$C_1''\,C_2'' = G''\,C_1'' + G''\,C_2'' - \frac{2\,C_1''\,G'' \times C_2''\,G''}{C_1''\,C_2''}\left\{\sin\frac{(\lambda_1 - \lambda_2)}{2}\right\}^2$$

The true length of the pendulum is

$$C_1''\,C_2'' + \frac{2\,C_1\,G \times C_2\,G}{C_1\,C_2}\left\{\sin\frac{(\lambda_1 - \lambda_2)}{2}\right\}^2 - 2\,G\,C_1\left\{\sin\frac{\vartheta'_1}{2}\right\}^2 - 2\,G\,C_2\left\{\sin\frac{\vartheta'_2}{2}\right\}^2$$

$$- t\sin\vartheta'_1 - t\sin\vartheta'_2$$

$$+ \frac{k^2(\sin\varepsilon_1^2 - \sin\varepsilon_2)^2 - k'^2(\cos\varepsilon_1^2 - \cos\varepsilon'_2)^2 - k''^2(\cos\varepsilon''_1{}^2 - \cos\varepsilon''_2{}^2)}{G\,C_2 - G\,C_1}$$

The sign of the quantity $t\sin\vartheta'$ depends upon which surface of the pendulum the distance between the axes is measured, and might be got rid of by measuring the distance between the knife edges on each of the surfaces which are intersected by them, and taking the mean.

I have endeavoured as far as possible to conform to the notation of M. Poisson in the Traité de Mécanique ; but this is rendered difficult, because M. Poisson sometimes takes the axis Ox to be vertical, (vol. ii. p. 113,) and sometimes the axis Oz (as vol. ii. p. 185), and he uses the letter s in two different acceptations, (vol. ii. pp. 119 & 185.)

In the notation of the article on the Pendulum in the Supplement to the Encyclopædia Britannica

$$\varepsilon = X,\ \varepsilon' = Y \text{ and } \varepsilon'' = Z,\ a = h.$$

The author of this article assumes the equations of the axis of rotation to be

$$x_1 = a\,z_1 + \alpha$$
$$y_1 = b\,z_1 + \beta$$

and he gives the equation $h = \frac{\sqrt{a^2+\beta^2}}{\sqrt{1+a^2+b^2}}$

this equation is incorrect; it should be

$$h = \frac{\sqrt{\{(a^2+\beta^2)(1+a^2+b^2)-(a\,a+b\,\beta)^2\}}}{\sqrt{(1+a^2+b^2)}}$$

It is easy by proper substitutions in the equations which I have given, to ascertain the influence of any deviation of the knife edge; and for this purpose I shall take the pendulum described in the Annals of Philosophy, vol. iv. p. 137. used by Mr. Baily, of which the length is 62 inches, the width 2 inches, and the thickness ·275 inch. One of the knife edges is 5 inches from the extremity; and therefore from well known expressions for the moment of inertia in a parallelepiped,

$$k^2 = \frac{31^2+1}{3} = 320{\cdot}6666$$

$$k'^2 = \frac{31^2+{\cdot}1375^2}{3} = 320{\cdot}339$$

$$k''^2 = \frac{1+{\cdot}1375^2}{3} = {\cdot}33964$$

If λ, δ, δ' and $\varepsilon_1 = 0$, $G\,C_1 = 11.2514$, $G\,C_2 = 28.5$, $C_1\,C_2 = 39.7514$

1. When δ, δ' and $\varepsilon_1 = 0$, the true length of the simple pendulum

$$= C_1''C_2'' + \frac{2\,C_1G \times C_2G}{C_1C_2}\left\{\sin\frac{\lambda}{2}\right\}^2$$

$$= 39{\cdot}7514 + {\cdot}80667\left\{\sin\frac{\lambda^2}{2}\right\}^2.$$

2. When λ, δ' and $\varepsilon_1 = 0$, the true length of the simple pendulum

$$= C_1''C_2'' + \frac{k^2-k'^2}{G\,C_2 - G\,C_1}\sin\delta^2$$

$$= 39.7514 + .013137\sin\delta^2.$$

3. When λ, δ and $\delta_1 = 0$, the true length of the simple pendulum

$$= C_1'' C_2'' + \frac{k^2 - k'^2}{G C_2 - G C_1} \sin \delta'^2 - 2 G C_1 \sin \frac{\delta'^2}{2}$$

$$= 39.7514 + 18.5712 \sin \delta'^2 - 22.5028 \sin \frac{\delta'^2}{2}.$$

4. When λ, δ and $\delta' = 0$, the length of the simple pendulum

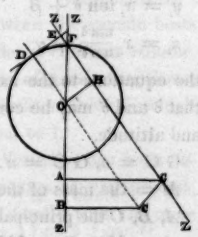
$$= \frac{C_1 C_2}{\cos \epsilon_1}.$$

The following table shows the increase in the number of vibrations in a day due to different values of λ, δ, δ', and t_1.

	λ	δ	δ'	t_1
30′	.62	.00	1.08	1.85
1°	2.66	.00	4.20	6.54

So that if, for example, $\lambda = 1°$, the number of vibrations in a day is increased by 2·66, and a deviation δ' in altitude of 1° has the effect of making the distance between the knife edges less than the true length of the simple pendulum by .00394, and of increasing the apparent number of vibrations by 4.20 in a day.

I shall now consider the case in which the plane is moveable with the pendulum, and rests upon the knife edge, which is fixed.



Let D E F be a section of the knife edge, F O $= r$ the angle D O F $= \theta$.

A G $= \bar{y}$, O A $= \bar{z}$, G H $= a$

$\bar{y} =$ B C + C G, ultimately when θ is small

$\bar{y} =$ B C $\sin \theta + r\theta$, O A = O B ultimately

$\bar{z} =$ B C $\cos \theta$.

It may easily be shown that

$$A \frac{d\theta^2}{dt^2} + M \left\{ \frac{d\bar{z}^2}{dt^2} + \frac{d\bar{y}^2}{dz^2} \right\} + 2 M g \bar{z} = M h$$

(See Poisson, Traité de Mécanique, vol. ii. p. 189.)

$$d\bar{y} = \{a \cos \theta + r\} d\theta$$

$$d\bar{z} = -a \sin \theta \, d\theta$$

$$\{k^2 + (a + r)^2\} \frac{d\theta^2}{dt^2} + 2 g a \cos \theta = h$$

The length of the simple pendulum is $\frac{k^2 + (a + r)^2}{a}$

If the two knife edges are isochronous, and $r_1 = r_2$

$$\frac{k^2 + (a_1 + r)^2}{a_1} = \frac{k^2 + (a_2 + r)^2}{a_2}$$

$$k^2 = \frac{a_1(a_2 + r)^2 - a_2(a_1 + r)^2}{a_2 - a_1}$$

The length of the simple pendulum $= a_2 + a_1 + 2r =$ the distance between the planes which vibrate on the knife edges.

Index to the notation.

Ox, Oy, Oz rectangular coordinate axes meeting in the point O. G the centre of gravity, Gx_ρ, Gy_ρ, Gz_ρ their principal axes which intersect each other in the point G. C the point in the axis Ox, where a perpendicular let fall upon it from the centre of gravity G cuts it, C′ the point when the plane (zy) cuts the axis Ox; C″ the point where one of the surfaces of the pendulum cuts the same knife edge, G″ the point in this surface where a perpendicular from G cuts it.

g the force of gravity

$y_1 = x_1 \tan\delta + \beta$

$z_1 = x_1 \frac{\tan\delta}{\tan\delta'} + \gamma$

the equations to the axis Ox referred to the coordinate axes Gx_ρ, Gy_ρ, Gz_ρ so that δ and δ' may be considered as the deviations of the knife edge in azimuth and altitude.

$G C = a$, $G C' = a'$.

$M =$ the mass of the pendulum.

A, B, C the principal moments of inertia.

$A = Mk^2$, $B = Mk'^2$, $C = Mk''^2$.

ϵ, ϵ', ϵ'' the angles which the line Ox makes with the coordinate axes, Gx_ρ, Gy_ρ, Gz_ρ.

$t =$ half the thickness of the pendulum.

$\beta = a' \sin\lambda$, $\gamma = a' \cos\lambda$.

$\bar{x}$, $\bar{y}$, $\bar{z}$ the coordinates of the centre of gravity.

r the radius of curvature of the knife edge.

XV.—*On the theoretical investigation of the velocity of sound, as corrected from M.* Dulong's *recent experiments, compared with the results of the observations of Dr.* Moll *and Dr.* Van Beek. *By Dr.* Simons, *Assistant at the Observatory of the University of Utrecht. Communicated by Captain* Henry Kater, *Vice-President.*

Read March 18, 1830.

It has been demonstrated by the ever-to-be-lamented Laplace*, that in order to obtain the velocity of sound by calculation, Sir Isaac Newton's original expression†,

$$V = \sqrt{\frac{g \cdot p}{D}},$$

must be multiplied by the square root of the ratio between the specific heats of atmospheric air under a constant pressure and under a constant volume. In this formula V is the velocity of sound, g the intensity of gravitating force, p the atmospheric pressure, and D the density of the medium through which sound is transmitted; the density of mercury being equal to 1.

The coefficient, which is to multiply the Newtonian formula, has been deduced by M. Laplace, first from MM. Laroche and Berardo's‡ experiments, next from those of MM. Clement and Desormes§, and finally from the more accurate investigations of MM. Gay-Lussac and Welter.

By introducing this correction, the velocity as deduced from calculation, was found to differ but little from what is actually obtained by experiment. But this difference between calculation and experiment, however small, was always such, that the observed velocity constantly exceeded that which was deduced by calculation.

* Annales des Physique et de Chimie, t. iii. p. 238. t. xxiii. p. 1. Mécan. Céleste, t. v. p. 119, *seqq.*

† Princip. l. ii. prop. 48.

‡ Annales de Chimie, t. lxxxv. p. 72.

§ Journal de Physique, t. lxxxix. p. 333, *seqq.*

The degree of accuracy with which experiments on the velocity of sound are now conducted, naturally led to a supposition that some of the elements of the theoretical formula were susceptible of a greater degree of correctness; and thus natural philosophers were rather inclined to attribute the difference between experiment and calculation to some deficiency of the analytical expression, than to error in the observation.

It has been shown in a recent volume of the Philosophical Transactions, that the experiments on the velocity of sound, made by Captain Parry in the polar regions, lead to the same conclusions with those made by Drs. Moll and Van Beek, under widely different circumstances; and this coincidence would tend to confirm our doubts as to the correctness of some of the elements of the computation.

A very able experimentalist, M. Dulong, lately published some experiments on the specific heat of the gases*, in the investigation of which he recurs to Laplace's analytical formula. M. Dulong's reasoning is nearly this: If it be admitted that the velocity of sound in atmospheric air is obtained by multiplying the Newtonian formula by the square root of the ratio between the two specific heats of the air, under a constant pressure, and under a constant volume, it must follow, that this ratio, or the coefficient with whose square root the original formula must be multiplied, may also be deduced from the velocity of sound as given by observation. Accordingly, M. Dulong computed this ratio or coefficient from actual observations on the velocity of sound, by the formula

$$K = \frac{V^2}{\frac{g \cdot p}{D}}$$

in which K is the ratio between the specific heat of air under a constant pressure, and the specific heat of air under a constant volume, whilst V is the velocity of sound as obtained by experiment.

The object of this paper is to compare the inquiries of M. Dulong with the experiments on the velocity of sound made by Drs. Moll and Van Beek, which were published in the Philosophical Transactions.

The following Table contains only such of the observations of Drs. Moll and Van Beek, in which the guns were fired on both stations exactly at the same

* Annales de Physique et de Chimie, t. xli. p. 113.

moment, and in which the interval of time between the flash and the report was correctly observed at both extremities of the base.

Date.	No. of the Observation.	Barometrical Pressure reduced to 0° Centigr. and corrected for capillarity. Metres p.	Tension of Aqueous Vapour in Metres T.	Temperature, Centigrade Thermometer. t.	Time elapsed between Flash and Report.
1823.				°	seconds.
June 27	1	0.74450	0.0095347	12.08	52.035
	3	0.74455	0.0095474	12.08	51.790
	4	0.74455	0.0094585	12.01	51.695
	5	0.74465	0.0096998	11.94	51.860
	6	0.74455	0.0095347	11.88	51.850
	8	0.74470	0.0094459	11.67	51.950
	9	0.74470	0.0093697	11.60	51.845
	12	0.74475	0.0093697	11.25	51.865
	13	0.74470	0.0092173	11.25	51.945
	17	0.74500	0.0093697	10.47	51.960
	18	0.74505	0.0092173	10.40	52.055
	19	0.74495	0.0092173	10.40	52.025
	23	0.74480	0.0089888	10.28	51.995
	24	0.74470	0.0088364	10.14	51.945
	25	0.74475	0.0088364	10.07	52.020
	26	0.74480	0.0084683	10.00	51.335
June 28	4	0.74830	0.0084638	11.89	52.020
	5	0.74775	0.0083921	11.09	51.525
	6	0.74800	0.0084638	11.53	52.245
	7	0.74820	0.0081763	11.25	32.315
	8	0.74815	0.0081763	10.78	52.215
	9	0.74815	0.0083159	10.42	52.675
	10	0.74835	0.0083921	10.28	51.175
	14	0.74810	0.0081763	10.14	52.445
	15	0.74810	0.0082524	10.42	51.965
	17	0.74815	0.0086206	10.97	51.875
	18	0.74805	0.0086968	10.89	52.240
	19	0.74805	0.0087730	11.12	52.080

Length of base, 17669 metres.

From this Table and the following data, the results obtained by M. Dulong were compared with Drs. Moll and Van Beek's experiments.

The weight of a cube centimetre of mercury at the temperature of 0° Centigrade, is 13.596152 grammes. This is according to the experiments of MM. Biot and Arago, and MM. Dulong and Petit's subsequent investigations. The weight of a cube centimetre of dry atmospheric air, at zero of Centigrade scale, and under the barometric pressure of $0^m.760$ is, according to the same observers, 0.001299541 grammes*.

* Biot, Traité de Physiq. Expér. et Mathém. t. i. p. 402, *seqq.* and p. 387.

Now, if the intensity of the gravitating force at Paris be g', whilst it is g at the place where the experiments are made, we have the weight of a cube centimetre of dry atmospheric air, under a pressure of 760*mm*, and at zero Centigrade, $0.001299541\ \frac{g}{g'}$.

Under the same circumstances the ratio of the density of air to that of mercury, will be:

$$D = \frac{0.001299541}{13.596152} \cdot \frac{g}{g'} = \frac{1}{10462.273} \cdot \frac{g}{g'}.$$

If the barometric pressure becomes p, the temperature t Centigrade, the tension of aqueous vapor T, the density of the air becomes

$$D' = \frac{p - \frac{3}{8}T}{10462.273 \times 0.760\,\{1 + 0.00375.t\}} \cdot \frac{g}{g}$$

substituting this value of D in the formula of the velocity of Sound, and observing that $g = g' \cdot \frac{g}{g'}$ we have,

$$V = \sqrt{\frac{10462.273 \times 0.760\,\{1 + 0.00375.t.\}p.g' \cdot \frac{g}{g'}}{\{p - \frac{3}{8}T\} \cdot \frac{g}{g'}}} \times \sqrt{K.}$$

or, $$V = \sqrt{10462.273 \times 0.760\,\{1 + 0.00375.t.\}\frac{g'.p}{p - \frac{3}{8}T}} \times \sqrt{K.}$$

This formula shows that V is independent of the latitude; and thus that the velocity of sound is not directly affected by the geographical position of the place of observation. From the late M. Borda's pendulum experiments, we have the intensity of gravity at Paris, $g' = 9.82827$.

From MM. Gay-Lussac and Welter's experiments, the value of K is deduced $K = 1.3748$.

From the more recent investigations of M. Dulong, we have $K = 1.421$.

Thus, taking K from MM. Gay-Lussac and Welter, the velocity of sound is

$$V = \sqrt{9.82827 \times 10462.273 \times 0.760\,\{1 + 0.00375.t.\}\frac{p}{p - \frac{3}{8}T}} \times \sqrt{1.3748}$$

and taking K from M. Dulong,

$$V = \sqrt{9.82827 \times 10642.273 \times 0.760\,\{1 + 0.00375.t.\}\frac{p}{p - \frac{3}{8}T}} \times \sqrt{1.421}$$

Finally, supposing V′ the velocity of sound observed at a temperature t and a tension T, and V″ the velocity of sound at zero Centigrade and dry air, we have

$$V'' = V' \times \sqrt{\frac{p - \frac{3}{8}T}{p\{t + 0.00375 . t\}}}$$

From these formulæ the experiments of Messrs. Moll and Van Beek are calculated; the results of which are contained in the following Table.

A Comparative Table of the Velocity of Sound, as deduced by calculation, and obtained by the experiments of Drs. Moll and Van Beek.

Date.	Number of experiments.	Veloc. calc. from the determination of K by MM. Gay-Lussac & Welter in 1″.	Velocity observed by Drs. Moll and Van Beek in 1″.	Diff. of Obs. and Calc. Veloc., taking K from MM. Gay-Lussac and Welter.	Velocity calculated from the determination of K by M. Dulong in 1″.	Difference of Observedand Calculated Velocity taking K from M. Dulong.	Value of K as deduced from the experiments of Drs. Moll and Van Beek.	Observed Velocity reduced to 0° Cent. and in dry air.	Diff. between the observed reduced Velocity, and the mean reduced Velocity.
		m	m	m	m	m		m	m
1823.									
June27	1	335.590	339.565	+ 5.025	341.182	−1.617	1.4065	331.327	−0.917
	3	339.477	341.172	1.695	345.133	−3.961	1.3885	329.083	−3.161
	4	335.599	341.799	6.200	341.192	+0.607	1.4260	333.497	+1.253
	5	335.519	340.711	5.192	341.110	−0.399	1.4176	332.515	+0.271
	6	335.469	340.777	5.309	341.059	−0.282	1.4186	332.629	+0.385
	8	334.818	340.721	5.303	340.397	−0.276	1.4187	332.634	+0.390
	9	335.287	340.810	5.523	340.873	−0.063	1.4204	332.842	+0.598
	12	335.075	340.678	5.603	340.559	+0.019	1.4211	332.924	+0.680
	13	334.292	340.154	5.862	340.646	−0.492	1.4169	332.424	+0.180
	17	334.604	340.055	5.451	340.180	−0.125	1.4199	332.783	+0.539
	18	334.527	339.435	4.908	340.101	−0.666	1.4154	332.252	+0.008
	19	334.527	339.631	5.104	340.101	−0.470	1.4170	332.444	+0.200
	23	334.453	339.827	5.374	340.026	−0.199	1.4193	332.709	+0.465
	24	334.360	340.154	5.794	339.932	+0.222	1.4228	333.122	+0.878
	25	333.933	339.663	5.730	339.498	+0.165	1.4224	333.067	+0.823
	26	334.245	340.219	5.974	339.814	+0.405	1.4243	333.301	+1.057
June28	4	335.358	339.663	4.305	340.946	−1.283	1.4103	331.652	−0.592
	5	334.893	342.927	8.034	340.474	+2.453	1.4415	335.032	+2.788
	6	335.164	338.200	3.036	340.749	−2.549	1.3998	330.605	−1.539
	7	334.971	337.554	2.583	340.553	−2.999	1.3961	329.973	−2.271
	8	334.688	338.395	3.707	340.262	−1.870	1.4053	331.075	−1.119
	9	334.482	335.440	0.958	340.056	−4.616	1.3827	328.386	−3.858
	10	334.404	345.272	10.868	339.976	+5.296	1.4622	338.090	+6.154
	14	334.301	336.911	2.610	339.872	−2.961	1.3963	330.004	−2.240
	15	334.481	340.023	5.542	340.054	−0.031	1.4207	332.874	+0.630
	17	334.840	340.613	5.773	340.419	+0.194	1.4226	333.094	+0.850
	18	334.798	338.233	3.435	340.377	−2.144	1.4031	330.808	−1.436
	19	334.941	339.272	+ 4.331	340.522	−1.250	1.4106	331.682	−0.562
						Mean number	1.4152	332.244	

The preceding table shows how very near M. Dulong's value of K agrees

with the result of experiment. Employing MM. Gay-Lussac's and Welter's co-efficient, the differences between the observed and calculated velocity are constantly affected with the same sign; whereas in taking K from M. Dulong, the differences are sometimes positive and sometimes negative. It is therefore presumed that M. Dulong's labours bring the calculation of the velocity of sound much nearer to the truth than before, and that such differences as are yet remaining between calculation and experiment, may be attributed, with great probability, to errors unavoidable in such complicated observations.

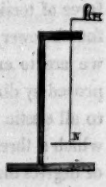
Utrecht, December 30, 1829.

XVI.—*On the elasticity of threads of glass, with some of the most useful applications of this property to torsion balances.* By WILLIAM RITCHIE, *A.M. F.R.S., Rector of the Royal Academy of Tain.*

Read March 18, 1830.

1. FROM facts connected with crystallization and elasticity, it seems extremely probable, that the atoms of matter do not attract each other indifferently on all sides. There appears to be peculiar points on their surfaces which have a more powerful attraction for each other, than for other points on the same molecule. This property is not peculiar to the atoms of ponderable matter, but seems also to belong to those of light and heat. It is as impossible to prove directly the existence of this property, as it is to prove the existence of atoms themselves; but on account of the satisfactory manner in which it enables us to explain the phænomena of crystallization and elasticity, it is now generally adopted.

2. If the atoms of solid bodies be slightly displaced by any mechanical means, they will endeavour to return to their former state of aggregation when the disturbing cause is removed. This property belongs in very different degrees to different substances. In lead it scarcely exists, and but slightly so in soft copper. In brass, iron and silver, especially when drawn into wires, it exists in a considerable degree. But all these substances have limits beyond which the property does not hold. If for example an iron wire be twisted several times, it will not return exactly to its former state, but remain partially twisted. In threads of glass, on the contrary, there seems to be no limit to this property, whilst the thread remains entire. Let a fine glass thread be suspended from a moveable index, and let a light horizontal needle of wood or straw be fixed to its lower extremity as in the annexed figure. If the thread be now twisted by means of the handle H, whilst the needle N is prevented turning round, and then allowed to untwist itself, the needle will return exactly to its former position after it has ceased to oscillate.



If the vitreous molecules be held together by the attraction of their poles or points of greatest affinity, it is obvious that these points will be displaced by torsion along the whole line of communication. The points of greatest attraction thus displaced, will therefore endeavour to regain their former state of stable equilibrium, and the thread will of course untwist itself till the needle returns to its former position. It would be curious to ascertain if a thread of glass, twisted as much as it can safely bear, and kept in that position for several months or years, would return exactly to its former position, or whether the atoms might not in course of time take up a new state of stable equilibrium.

3. The number of times a thread of glass may be twisted without breaking, will of course depend on its length and diameter. It is almost incredible the number of times a thread of a substance so brittle as glass may be twisted, before the points of greatest attraction of the vitreous molecules be actually removed beyond the sphere of attraction, or in other words, before the thread be broken. I have succeeded in drawing threads of glass of such extreme tenuity, that one of them, not more than a foot long, may be twisted nearly a hundred times without breaking. Hence it is obvious, that if a thread could be drawn so fine as to consist of a single line of vitreous molecules, torsion could have no tendency to displace the points of greatest attraction, and this elementary thread might be twisted for ever without breaking. In that case the compound molecules of glass would only turn round their points of greatest attraction, like bodies revolving on a pivot.

4. It is difficult to prove by direct experiment some of the laws of torsion established by Coulomb, as belonging to metallic wires, on account of the difficulty of procuring threads of glass of a uniform diameter throughout their whole length. It is difficult, for example, to prove by experiments, that the force of torsion of a glass thread is directly as its length, and inversely as the fourth power of its diameter. Fortunately, however, the only property which we are to employ in the construction of the following instruments, can be proved by direct experiment. This property is, within certain limits, common to all elastic threads, namely, "that the force of torsion, or that force with which a thread tends to untwist itself, is directly proportional to the number of degrees through which it has been twisted*."

* Biot, Traité de Physique, tom. i. p. 486.

This property may be established by the following methods.

1st, Let a horizontal needle of glass, or of any substance not magnetic, be fixed to the lower extremity of a fine thread of glass, and then made to oscillate: it will be found that these oscillations are isochronous, even when the thread has been twisted through several circumferences. Now this isochronism may be proved to belong only to an elastic thread possessing the property enunciated in the preceding proposition.

2ndly, Suspend a magnetic needle in a horizontal position, by a similar glass thread, over the centre of a large circle having its circumference divided into degrees. Twist the thread by means of the key as in the common torsion balance, and note the degrees of torsion and the corresponding deviations of the needle from the magnetic meridian, and it will be found that the sines of the deviations are proportional to the corresponding degrees of torsion;—a property which can only belong to elastic threads possessing the property in question*.

5. This perfect elasticity of torsion belonging to threads of glass, may be applied with decided advantage to the electric and magnetic balances of Coulomb. All that is necessary in those cases is to substitute a thread of glass of the proper degree of fineness, for the silver wires employed with so much success by the ingenious inventor. The application of this property to the construction of a galvanometer and delicate balance, is, I believe, new, and therefore I have ventured to lay a description of them before the Society; but before doing this, it may not be improper to describe the best method of making the glass threads employed in the construction of these instruments.

Heat the end of a clean thermometer tube at the flame of a blowpipe, and draw it out till it be of the thickness required for fixing in the hole in the end of the torsion key, as in the annexed cut.

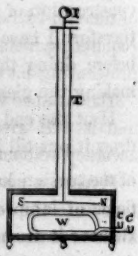
Direct the flame of lamp on the tube at A, till the glass has become sufficiently soft. Remove it from the flame, and draw it out rapidly till it be of the length and fineness required. By separating the thread from the tube, we shall thus have a thread of any degree of fineness, terminated by two thicker portions, which may be securely fixed with cement or sealing-wax as circumstances may

* Biot, Traité de Physique, tom. iii. p. 29.

require. Simple as this may appear, it requires some practice in manipulation to do it well; it will therefore be necessary to draw several threads of different lengths and degrees of fineness, and afterwards select those best adapted to the instrument to which it is to be applied.

6. Description and use of the torsion galvanometer.

Take a fine copper wire and cover it with a thin coating of sealing-wax. Roll it about a heated cylinder an inch or two in diameter, ten, twenty, or any number of times, according to the delicacy of the instrument required. Press together the opposite sides of the circular coil, till they become parallel and about an inch or an inch and a half long. Fix the coil in a proper sole, and connect the ends of the wires with two small metallic cups for holding a drop of mercury. Paste a circular disc of paper, divided into equal parts, horizontally on the upper half of the coil, and having a black line drawn through its centre and in the same direction with the middle of the coil. Fix a small magnet, made of a common sewing-needle or piece of steel wire, to the lower end of a fine glass thread, whilst the upper end is securely fixed with sealing-wax in the centre of a moveable index, as in the common torsion balance. The glass thread should be inclosed in a tube of glass, which fits into a disc of thick plate glass, covering the upper side of the wooden box containing the coil and magnetic needle.



The whole will be obvious from the simple inspection of the annexed vertical section of the instruments, in which W is the coil of wire, C C' the cups, T the glass tube containing the thread, I the index turning in the centre of a divided circle to mark the degrees of torsion, and N S the magnetic needle.

By means of this instrument we may compare with great accuracy the relative quantities of currents of voltaic electricity circulating along the wires of the coil. For this purpose place the needle directly above the line drawn on the paper, and consequently directly above, and in the direction of the wires forming the upper side of the coil. Cause a current of voltaic electricity to circulate along the wires, and the needle will of course be deflected. Twist the glass thread, by turning the index, till the needle be brought to its former position, and note the number of degrees of torsion; untwist the thread, and

repeat the experiment with another current; and the quantities of electricity circulating round the wires will be directly proportional to the number of degrees through which the thread has been twisted. By this contrivance it is obvious that the currents always act with the same mechanical advantage on the needle; and consequently their deflecting forces, which are counterbalanced by the elastic force of torsion, must be directly as the number of degrees through which the thread has been twisted. In the common galvanometer, the deflecting force acts with diminished mechanical advantage as the needle deviates from the coil. When it has been deflected nearly 90 degrees from the original position, an additional power will produce scarcely any effect, and consequently the instrument ceases to give indication of a more energetic current.

This instrument appears to me well adapted to many interesting investigations connected with voltaic electricity; but these could not properly be introduced in this paper, and may therefore form the subject of another communication.

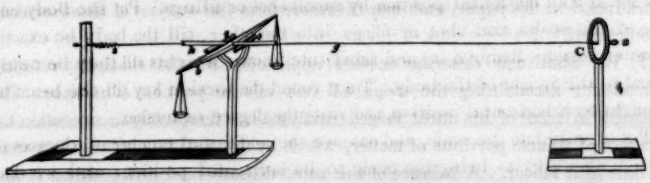
7. We shall now describe another application of the elasticity of glass threads for ascertaining the weight of very minute portions of matter. The chemist in some of his most refined analyses has frequently to ascertain the weights of minute portions of matter, which by the ordinary process is a work of time and labour. A balance of the most delicate and perfect kind is also required, which, from its expense, frequently prevents the young chemist from prosecuting his experimental researches.

The balance which I am now to propose may be made at a trifling expense, and it will give the weights of small quantities of matter to a degree of accuracy seldom attained by the most perfect hydrostatic balances.

Prepare a small wooden beam, very light and about a foot or fifteen inches long, into the centre of which fix a steel blade having a smooth edge, like the blade of a fine penknife. To one extremity of the blade and in the line of its edge a fine thread of glass is to be securely fixed, whilst the other end of the thread is to be secured with sealing-wax in the centre of a small cylindrical key, passing through the centre of a vertical circle divided into degrees, or any convenient number of equal parts. To the other end of the fulcrum, and also in the line of the edge, a fine thread formed of a few fibres of untwisted silk is

to be attached, whilst its other extremity is to be fixed to a spiral spring formed of fine brass wire, in order to keep the glass thread properly stretched. The knife edge is made to rest on two small portions of thermometer tube, placed parallel to each other on the top of an upright support. Portions of a still smaller steel blade are to be fixed, nearly in the same straight line with the edge of the fulcrum and at the ends of the beam, for suspending the scales. In one end of the beam a fine sewing-needle is to be fixed, for the purpose of pointing out, on a divided scale, the position of the beam when nearly horizontal. A similar needle is to be fixed in the cylindrical key, for pointing out the number of degrees of torsion which the thread has suffered.

The annexed figure represents a perspective view of the instrument seen obliquely, in which b is a beam, g the glass thread, k the knife edge fulcrum, $t\,t'$ the thermometer tubes for supporting the beam, s the spiral spring with its attached silk thread, c the divided circle, and a the torsion key.



Having thus described the balance itself, we shall now explain the manner of employing it; first, for the determination of very small weights, and secondly, for heavier ones.

1st. To determine the weight of a small quantity of matter by employing only one weight, suppose a grain.—Twist the glass thread by turning the torsion key through two or three circumferences, according to the degree of torsion which it will bear without the risk of breaking. Put brass filings or other convenient counterpoise, into one of the scales till the beam be brought to a position nearly horizontal, the index of the torsion key pointing to zero on the divided circle. Place the body to be weighed into the scale, which is to be raised by untwisting the glass thread. Turn the torsion key till the elastic force of the thread raise the weight, and carefully observe the point on the scale at which the needle, in the end of the beam, becomes stationary. Note

the number of degrees of torsion which the thread has suffered. Remove the weight, and untwist the thread till the beam returns to its horizontal position. Put a small known weight into the same scale, and turn the torsion key till the beam be raised to its former horizontal position, and observe the number of degrees of torsion:—then will the degrees of torsion give the ratio of the known and unknown weights. For example, if the thread suffered a torsion of 1500 degrees to raise the body B, and 1000 to raise one grain, then 1000 : 1500 : : 1 grain : 1.5, the weight of the body. If the body required only 50 degrees of torsion to raise it, then its weight would be $\frac{50}{1000}$ or .05 of a grain.

2ndly. When the body to be weighed is much heavier than a grain, the best way will be to ascertain its weight within a grain by the method of double weighing, and then apply the principle of torsion for ascertaining the fraction of a grain. Suppose the body to weigh nearly 100 grains, twist the thread through two or three circumferences exactly as in the former method. Bring the beam to a horizontal position by small shot or filings. Put the body into one of the scales, and shot or filings into the other, till the body be exactly counterpoised. Remove it, and substitute known weights till they be nearly equal to the weight of the body. Turn round the torsion key till the beam be brought to a horizontal position, and note the degree of torsion.

Put a grain into the scale, and observe the additional number of degrees of torsion necessary to bring the beam to its horizontal position; and we thus get the fraction of a grain which the body exceeds the known weights employed. If for example the body weighed nearly 100 grains, and it required a torsion of 50 degrees to raise the scale when 99 grains had been put into it after the body had been removed, and also that the thread required an additional torsion of 1000 when one grain had been placed in the same scale, then the weight of the body will be $99\frac{50}{1000}$ or 99.05 grains.

8. It is of course necessary to prevent the agitation of the air acting on the balance and its scales, and therefore the whole may be inclosed in a box as in the common balance. It is not necessary, however, to inclose the glass thread and the divided circle, and therefore the thread may be made to pass through a hole in the back of the box, and removed when the balance is not in use. It is useful to have a number of threads of glass of different degrees of

fineness, having small brass tubes cemented on their ends for the purpose of attaching them to the fulcrum and torsion key.

The method now described may appear somewhat tedious, but it is only so in appearance, as the oscillations do not continue so long as in a delicate balance without the torsion thread. In some delicate experiments with this balance, I have used threads of glass about ten feet long, so that in raising a weight of one grain, the glass thread suffered a torsion of at least 5000 degrees. Hence a very small fraction of a grain may be determined with an extraordinary degree of accuracy. From the perfect elasticity of torsion which glass possesses, and from the ease with which threads of any length and fineness may be procured, I am fully convinced that, for all delicate investigations connected with torsion balances, threads of this substance will be found to possess decided advantages.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

MDCCCXXX.

METEOROLOGICAL JOURNAL FOR JULY, 1829.

1829. July.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
● ☿ 1	29.647	65.3	29.513	66.9	55	59.7	61.8	57.6	65.5	0.292	SSE	Overcast—rain.
☿ 2	29.587	73.5	29.631	68.7	51	64.6	66.7	55.7	68.7	0.007	SSW	Fine and clear—brisk wind.
♀ 3	29.609	67.8	29.477	67.7	56	64.5	69.8	57.0	67.8		SSE	Cloudy—showery. Rain, with high wind, at night.
♄ 4	29.631	73.5	29.573	67.8	50	63.5	58.7	52.8	68.6	0.036	WSW	A.M. Fine and clear. P.M. Overcast.
☉ 5	29.585	68.1	29.660	67.1	51	58.4	60.7	50.7	68.7	0.089	W	Cloudy. Evening fine and clear.
☽ 6	29.876	69.7	29.893	69.3	55	66.0	69.7	52.8	71.7	0.125	W	Fine and clear—light wind.
♂ 7	29.878	65.7	29.741	65.9	60	60.7	60.7	55.8	67.7	0.042	WSW	Overcast—brisk wind. Rain.
☿ 8	29.834	70.3	29.786	70.7	55	67.3	69.3	55.8	71.7	0.044	NW	Fine—light clouds.
♃ 9	29.741	67.7	29.809	68.3	51	63.2	65.7	56.7	67.7		NW	Fine—cloudy.
♀ 10	29.913	70.6	29.817	70.2	50	66.7	65.0	53.4	68.8		SW	A.M. Fine. P.M. Lowering. Evening, rain.
♄ 11	29.556	66.4	29.485	67.1	61	62.3	61.6	58.9	65.8	0.022	SSE	Lowering. P.M. Heavy rain.
☉ 12	29.422	67.7	29.497	69.0	62	63.7	68.0	56.7	69.2	0.514	WSW	Fine and clear—cloudy.
☽ 13	29.658	68.3	29.733	70.3	60	63.7	68.5	56.4	69.5	0.100	SSW	Lowering.
♂ 14	29.889	69.2	29.861	70.3	66	66.4	68.7	62.8	73.7		SSW	Cloudy. Heavy rain A.M.
☿ 15	30.014	74.6	29.982	73.0	60	68.7	73.5	58.3	74.7	0.125	W	Fine and clear—light clouds.
○ ♃ 16	29.947	74.3	29.958	73.9	56	66.9	69.5	57.8	73.2		W	Fine. Heavy showers P.M.
♀ 17	29.910	68.3	29.683	68.0	60	64.1	60.3	56.4	64.8	0.125	SSE	Lowering—light wind. Rain P.M.
♄ 18	29.562	68.7	29.589	70.3	64	64.3	66.7	59.7	69.8	0.239	SE	A.M. Lowering—heavy showers. P.M. Fine.
☉ 19	29.785	70.2	29.867	71.7	60	65.9	66.4	59.8	70.2	0.222	NW	Fine—lightly cloudy.
☽ 20	30.097	73.0	30.088	71.3	54	66.3	70.6	51.7	71.1	0.047	NW	Fine and clear.
♂ 21	30.254	72.3	30.238	71.6	57	66.9	73.8	54.0	74.7		WSW	Fine—light clouds.
☿ 22	30.260	74.3	30.205	73.7	62	69.8	76.2	59.3	78.3		W	Fine and clear—cloudy.
♃ 23	30.187	68.7	30.153	72.4	62	63.7	71.4	60.7	73.3		W	A.M. Overcast. P.M. Fine.
♀ 24	30.030	75.2	29.954	74.3	66	68.0	71.7	59.3	72.0		E	Fine—cloudy.
♄ 25	29.873	70.6	29.897	74.5	65	65.8	74.6	64.2	75.4	0.183	WSW	A.M. Thunder and lightning, from 1h to 2h, with hail and rain.
☉ 26	30.028	65.3	30.060	65.6	57	57.8	58.9	56.0	60.6		N	Overcast. Shower at noon.
☽ 27	30.101	65.1	30.102	66.9	56	59.3	64.0	48.7	66.8	0.008	NNE	Fine—light clouds.
♂ 28	30.098	70.7	29.999	67.7	51	66.3	67.9	51.7	70.3		WSW	Fine and clear.
☿ 29	29.642	64.2	29.664	68.0	59	60.1	69.2	58.4	69.7	0.153	SE	A.M. Showers, early. Fine—cloudy.
● ♃ 30	29.748	69.6	29.755	69.3	53	64.3	67.7	51.5	69.7		NNE	Fine—light clouds. Thunder and lightning at 4 P.M. with heavy rain.
♀ 31	29.977	60.7	30.070	66.7	58	58.5	64.0	52.3	65.3	0.347	NNW	A.M. Cloudy. P.M. Clear—brisk wind.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.850	69.1	29.830	69.6	57.5	64.1	66.9	56.2	69.8	2.910		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.760 } { 3 P.M. 29.737 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

..... above the mean level of the Sea (presumed about)..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR AUGUST, 1829.

1829. August.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♄ 1	30.248	67.5	30.242	67.6	63	63.3	66.0	51.3	68.7		NNW	A.M. Fine and clear. P.M. Lowering —distant thunder.
☉ 2	30.289	66.7	30.262	68.7	57	65.7	72.7	54.3	73.7		W	Fine and clear—light clouds and wind.
☽ 3	30.122	66.8	29.965	66.7	59	65.5	63.0	57.7	67.7		SW var.	Rain—light brisk wind.
♂ 4	29.826	67.8	29.791	66.8	55	61.0	62.6	50.7	64.4	0.183	W	Cloudy—brisk wind. Showery P.M.
☿ 5	29.912	63.7	30.010	66.7	57	61.8	65.9	53.8	65.8	0.039	NNW	Fine—light clouds and breeze. P.M. Cloudy.
♃ 6	30.078	64.0	30.070	68.2	60	62.7	66.7	54.3	69.4		W	Lowering.
♀ 7	30.184	64.7	30.207	68.9	61	61.7	70.6	60.3	71.7	0.114	E	A.M. Light rain, early. P.M. Fine.
♄ 8	30.240	68.4	30.188	71.4	64	67.0	76.5	58.7	76.5		E	Fine and clear—light clouds.
☉ 9	30.115	67.4	30.041	71.2	66	66.3	71.2	57.6	71.7		SSW	A.M. Cloudy. P.M. Clear—light clouds. Breeze.
☽ 10	29.935	66.2	29.921	69.3	60	60.7	69.3	60.5	70.2	0.406	W	A.M. Heavy rain, early. P.M. Fine and clear.
♂ 11	30.123	64.7	30.144	67.4	53	61.7	67.7	54.8	69.3	0.112	W	Fine—light clouds.
☿ 12	30.137	65.8	30.065	69.4	61	65.7	70.7	58.3	71.8		S	Fine and clear—light wind.
♃ 13	29.778	66.3	29.738	69.7	66	66.2	67.3	61.4	72.8		SSW	A.M. Fine. P.M. Lowering. Light wind.
○ ♀ 14	29.505	65.7	29.452	68.2	64	64.4	68.2	61.3	69.8	0.007	SE	Cloudy—heavy rain.
♄ 15	29.577	60.8	29.628	61.8	56	56.5	57.8	53.3	60.4	0.180	W var.	Rain. Light brisk wind.
☉ 16	29.967	58.7	30.053	61.7	54	57.6	61.0	51.0	63.6	0.292	NNW	Fine and clear—cloudy. Brisk wind.
☽ 17	30.180	56.8	30.155	63.7	51	57.0	65.3	46.7	66.4		W	A.M. Fine & cloudless. P.M. Broken clouds.
♂ 18	29.847	59.3	29.753	62.7	58	58.3	62.4	55.4	64.3	0.092	SSW	Overcast—rain. Light wind.
☿ 19	29.526	63.7	29.476	65.3	60	63.7	66.3	58.0	67.3	0.147	WSW	Fine and clear—cloudy. Showery P.M.
♃ 20	29.284	60.7	29.500	63.7	59	59.7	61.3	55.7	63.3	0.063	SSW	Rain. Evening clear.
♀ 21	29.841	60.0	29.902	62.3	52	63.3	62.6	51.8	63.3	0.114	NNW	Fine—light clouds.
♄ 22	29.924	58.7	29.791	62.3	53	59.3	61.4	52.2	63.4		S	Cloudy. At night, strong high wind with heavy rain.
☉ 23	29.514	63.8	29.504	67.5	63	63.0	66.2	59.5	68.3	0.286	SSW	Cloudy—brisk wind. Rain A.M.
☽ 24	29.347	63.3	29.525	63.6	59	60.4	62.7	59.3	62.9	0.128	W	Lightly cloudy and showery.
♂ 25	29.922	58.7	29.999	62.3	52	58.0	62.5	49.7	63.4	0.161	WSW	Fine and clear—brisk wind.
☿ 26	30.060	59.6	29.914	62.6	57	59.3	61.2	51.3	62.7		S	Cloudy. At night, strong wind.
♃ 27	29.626	62.4	29.560	63.3	48	60.4	60.9	57.8	65.7	0.008	WSW	Fine and clear. Evening, brisk wind.
♀ 28	29.521	58.4	29.650	61.6	56	58.4	60.3	53.8	61.0	0.272	NW	Light clouds and brisk wind. At night, rain and strong wind.
● ♄ 29	29.918	60.0	30.499	64.3	59	59.0	64.0	54.7	64.2	0.125	N	Clear—light clouds—strong brisk wind.
☉ 30	30.193	57.2	30.173	61.0	52	55.7	61.3	52.3	62.3		NNW	Fine and clear—light wind.
☽ 31	30.100	56.7	30.068	60.7	56	56.3	59.7	53.3	60.7		NNW	Cloudy—light wind.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.898	62.7	29.911	65.5	57.8	61.3	65.0	55.2	66.6	2.839		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.833 | 3 P.M. 29.898 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.

..... above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR SEPTEMBER, 1829.

1829. Septemb.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♂ 1	29.998	58.3	29.965	64.0	57	57.4	63.5	54.3	64.2	0.044	NNE	Cloudy—light wind. Evening, light rain.
☿ 2	29.955	60.6	29.980	65.6	59	59.7	63.8	56.4	66.5	0.008	NNW	Cloudy and foggy. Evening clear.
♃ 3	30.176	57.4	30.510	61.3	49	55.3	62.4	52.2	63.2		NW	Fine and clear—lightly cloudy.
♀ 4	30.009	58.0	29.945	61.7	50	56.6	62.0	53.6	63.3		W	Fine and clear—cloudy.
♄ 5	29.712	58.2	29.581	62.0	56	58.3	60.3	51.9	63.5		SSE	Overcast—light wind. At night, light rain.
☉ 6	29.531	61.7	29.499	63.8	59	60.8	63.3	54.8	65.2	0.005	WSW	Fine—lightly overcast.
☽ 7	29.663	59.8	29.658	64.3	54	58.5	63.7	52.3	64.3	?	S	Fine and clear—light clouds. Showery P.M.
♂ 8	29.496	61.3	29.563	63.4	59	61.3	60.7	54.4	65.2	?	S var.	Heavy rain, early A.M. Fine and clear—brisk wind.
☿ 9	29.671	59.7	29.682	64.2	55	58.7	64.2	52.7	65.5	0.389?	WSW	Fine and clear—light wind.
♃ 10	29.387	62.7	29.357	66.5	61	62.0	66.7	58.8	66.7	0.389?	SE	A.M. Cloudy. P.M. Fine and clear. Light brisk wind.
♀ 11	29.606	60.0	29.689	64.6	55	59.4	62.8	55.4	65.2	?	WSW	Fine and clear. Evening rainy.
♄ 12	29.663	59.2	29.630	63.3	54	57.3	60.3	50.3	63.7	0.203	W	Fine and clear—cloudy. Light showers.
○ ☉ 13	29.565	56.0	29.442	62.4	53	55.7	61.4	47.5	62.7	0.011	SSW	Fine and clear. Heavy rain at night.
☽ 14	29.208	57.3	29.200	61.6	48	54.4	56.6	52.4	61.6	0.556	W	A.M. Fine. P.M. Showery.
♂ 15	29.668	53.7	29.765	60.4	45	53.0	59.1	44.9	61.3	0.050	W	Very clear. P.M. Showery: at 4h. thunder and lightning.
☿ 16	29.561	54.7	29.535	57.2	52	52.5	54.7	49.5	54.7	0.408	N	Foggy. Light brisk wind.
♃ 17	29.868	53.3	29.807	59.2	50	50.6	57.9	44.8	58.3	0.033	WNW	Overcast. At night, rain.
♀ 18	29.175	56.7	29.161	61.2	56	56.7	59.7	45.0	63.8	0.389	ESE	Fine—lightly cloudy.
♄ 19	29.472	56.7	29.631	56.8	54	54.5	55.2	51.7	56.7		NW	Overcast.
☉ 20	29.929	55.9	29.898	58.7	51	51.7	58.4	45.7	58.8	0.089	NW	Fine—light clouds.
☽ 21	29.761	56.7	29.798	61.3	54	56.4	61.0	51.4	62.3	0.011	W	Fine and clear—light clouds.
♂ 22	29.880	55.7	29.812	60.5	53	55.7	60.0	48.4	61.4	0.056	WSW	Fine—lightly cloudy.
☿ 23	29.834	56.7	29.844	60.6	53	53.9	59.4	51.2	62.7	0.019	WSW	Fine—hazy.
♃ 24	29.957	55.7	29.913	59.6	51	53.7	58.2	51.6	59.7		WSW	A.M. Strong haze. P.M. Fine.
♀ 25	30.125	53.7	30.146	58.7	53	52.7	58.4	48.3	58.8		N	Fine. Strong fog at night.
♄ 26	30.232	51.6	30.168	59.4	49	51.2	59.3	46.2	62.0		N	Strong haze.
☉ 27	29.892	60.7	29.797	61.3	58	61.8	60.3	50.4	62.0		WSW	Fine and clear—light wind. Showers at noon.
● ☽ 28	29.876	52.9	29.873	58.0	46	51.4	54.2	46.3	56.7	0.044	NW	Fine—light clouds.
♂ 29	29.983	51.0	29.986	54.6	42	48.6	54.0	44.3	54.3		N	Fine—light clouds.
☿ 30	30.213	50.2	30.212	54.8	43	44.3	54.2	42.4	54.3		N	A.M. Strong fog. P.M. Fine & clear.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.769	56.9	29.772	61.0	52.6	55.5	59.8	50.3	61.6	2.704?		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.709 } { 3 P.M. 29.709 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 9½ in.

..... above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR OCTOBER, 1829.

1829. October.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock P.M. Barom.	3 o'clock P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♃ 1	30.252	52.0	30.220	58.8	51	53.0	59.6	43.6	59.7		ENE	Fine—light brisk wind.
♀ 2	30.040	56.7	29.966	58.6	53	57.4	57.8	51.9	58.8		E	Overcast—showery. Brisk wind A.M.
♄ 3	29.840	58.9	29.788	58.6	55	58.8	56.8	54.3	58.8	0.131	SSW	Overcast. Heavy showers P.M.
☉ 4	29.968	53.6	29.961	58.4	52	53.0	57.7	47.7	58.3	0.444	W	A.M. Fine. P.M. overcast.
☽ 5	29.571	56.7	29.570	59.1	54	54.8	56.8	52.7	59.4	0.056	W	A.M. Rain. P.M. Fine.
♂ 6	29.670	51.4	29.675	54.2	46	50.2	52.5	44.6	53.4	0.072	NW	Fine—light haze.
☿ 7	29.509	49.0	29.443	40.2	38	46.5	37.4	42.3	46.5		W	Overcast. Rain and snow P.M.
♃ 8	29.808	43.8	29.921	46.9	27	39.5	47.5	33.3	47.5	0.806	W	Fine and cloudless. A.M. Strong wind early.
♀ 9	30.203	41.8	30.229	47.5	31	40.3	47.0	34.3	47.0		NW	Fine and cloudless—hazy.
♄ 10	30.432	44.0	30.428	50.4	41	43.7	54.7	35.7	56.0		WSW	Overcast and hazy.
☉ 11	30.317	51.7	30.211	55.1	49	52.6	57.7	43.0	57.7		W	Cloudy—light wind.
○ ☽ 12	30.152	54.7	30.138	56.2	48	54.7	56.2	52.2	56.3		NW	A.M. Cloudy. P.M. Fine.
♂ 13	30.033	55.2	29.873	57.2	49	55.4	56.8	50.3	57.5		SW	Lightly cloudy.
☿ 14	29.420	56.7	29.578	51.0	51	52.8	48.8	52.3	52.8	0.078	NNW	Cloudy—light rain.
♃ 15	30.227	43.7	30.259	48.8	34	41.7	48.5	37.3	47.7	0.025	N var	Fine and cloudless—hazy.
♀ 16	30.075	48.6	29.944	51.4	39	47.3	51.0	39.5	54.1		SSW	Overcast.
♄ 17	30.047	52.3	30.119	55.5	45	51.3	55.4	46.8	55.7		NW	Fine—light breeze.
☉ 18	30.127	55.3	30.129	59.0	54	56.0	61.1	50.8	61.4		WSW	A.M. Cloudy. P.M. Cloudless—light wind.
☽ 19	30.080	57.7	30.050	60.8	57	58.2	59.8	53.3	60.5		SW	A.M. Cloudy. P.M. Clear—light clouds.
♂ 20	29.873	58.8	29.808	60.5	56	57.5	59.4	55.5	60.2		SW	Clear—light clouds.
☿ 21	29.805	57.0	29.703	58.6	53	53.8	57.0	48.3	57.7	0.017	S	A.M. Clear & cloudless. P.M. Cloudy. Evening rainy.
♃ 22	29.658	54.4	29.728	55.1	42	51.4	53.0	48.6	53.7	0.100	W	A.M. Cloudy. P.M. Fine.
♀ 23	29.955	49.7	29.934	51.6	41	44.8	49.8	41.7	49.8	0.019	NNW	Fine—hazy.
♄ 24	29.957	46.7	29.913	51.5	43	45.7	51.2	39.8	51.3		NNE	Fine—light clouds.
☉ 25	30.141	45.4	30.197	52.8	42	44.3	51.8	40.2	51.8		NNW	Fine—hazy.
☽ 26	30.350	50.0	30.344	51.5	48	48.7	51.0	43.7	50.7		NNE	Hazy.
● ♂ 27	30.367	48.3	30.322	49.5	41	41.7	49.5	40.2	49.7		NNW	A.M. Foggy. P.M. Cloudless—hazy.
☿ 28	30.245	50.8	30.278	51.2	48	48.6	48.6	41.3	49.7		NNE	Clear—light clouds and wind.
♃ 29	30.347	43.7	30.275	47.5	36	42.1	45.4	39.2	45.4		NNE	Fine—hazy.
♀ 30	30.002	46.7	30.499	53.0	43	46.3	52.2	39.9	53.1		WSW	Lightly cloudy—hazy. Rain P.M.
♄ 31	29.917	51.7	29.990	45.7	44	46.9	49.8	45.4	49.8		NNE	A.M. Overcast. P.M. Clear—light clouds.
	Mean 30.016	Mean 51.1	Mean 30.016	Mean 53.4	Mean 45.5	Mean 49.6	Mean 53.0	Mean 44.8	Mean 53.9	Sum 1.548		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.971 } { 3 P.M. 29.965 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

..... above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR NOVEMBER, 1829.

1829. Novem.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest.	Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☉ 1	30.157	43.8	30.171	44.5	27	35.7	41.8	31.7	41.8	0.025	NW	Cloudless—hazy.
☽ 2	30.196	41.7	30.212	46.8	36	39.3	47.2	32.8	47.2		WSW	Fine—hazy.
♂ 3	30.209	45.9	30.132	47.6	38	43.6	47.2	38.3	47.3		WSW	Lightly cloudy—hazy.
☿ 4	29.803	48.7	29.647	51.0	45	48.2	50.8	43.0	51.3		S	Overcast and foggy. Rain P.M.
♃ 5	29.967	48.8	30.267	51.5	43	43.2	50.4	39.7	50.4	0.131	W	Nearly cloudless.
♀ 6	29.978	50.0	29.877	53.6	45	48.3	52.0	42.3	52.7		WSW	Lightly cloudy—hazy.
♄ 7	29.908	49.3	29.884	51.2	42	45.2	48.8	42.8	48.8	0.011	WNW	Nearly cloudless—hazy.
☉ 8	30.017	42.5	30.015	49.6	38	38.5	47.0	34.6	46.6		WSW	Fine—lightly cloudy—light wind.
☽ 9	30.046	47.7	30.061	49.6	43	43.8	48.4	37.4	48.4		W	Fine—lightly cloudy—light wind.
♂ 10	30.007	48.4	29.866	51.2	43	43.3	49.5	43.3	52.4		SSW	Lightly cloudy.
○ ☿ 11	29.964	48.6	30.018	51.4	42	42.4	48.8	40.3	52.2		W	Strong fog.
♃ 12	29.884	51.6	29.839	55.6	52	52.7	56.4	41.2	56.4	0.278	W	Overcast—foggy.
♀ 13	30.052	54.6	30.160	52.4	50	50.4	47.0	49.8	50.4	0.006	E	Overcast—foggy.
♄ 14	30.185	51.4	30.117	51.8	43	44.1	46.8	41.4	47.8	0.006	E	Overcast—foggy.
☉ 15	29.890	51.8	29.822	53.6	48	48.5	51.8	42.7	51.8		WSW	Lightly cloudy. Light rain.
☽ 16	30.169	46.3	30.247	48.0	31	38.4	41.2	35.4	41.4	0.011	N	Clear and cloudless—light wind.
♂ 17	30.377	42.6	30.314	45.8	34	34.8	41.4	31.3	41.4		NNE	A.M. Cloudless. P.M. Light clouds.
☿ 18	30.387	43.7	30.388	46.2	38	38.3	41.8	33.8	41.8		NNE	Light clouds and haze.
♃ 19	30.428	39.7	30.364	40.5	32	32.3	35.5	30.3	35.5		NNE	Strong fog.
♀ 20	30.352	39.2	30.300	40.2	30	30.3	36.4	27.5	36.4		NW	Very dense fog. Hoar frost A.M.
♄ 21	30.286	37.9	30.204	39.8	32	32.4	38.6	26.8	38.6		NW	A.M. Strong fog. P.M. Fine.
☉ 22	29.662	39.2	29.477	41.8	38	38.3	42.0	31.3	42.0		SSW	Cloudy—light rain.
☽ 23	29.603	40.7	29.103	42.5	38	40.3	40.8	36.2	41.7	0.064	NNE	Light clouds. Rain P.M.
♂ 24	29.682	40.6	29.016	40.6	33	39.2	38.0	36.4	38.8	0.019	NE	Overcast—light brisk wind.
☿ 25	29.526	37.2	30.109	37.2	32	31.9	33.0	29.7	36.3		NE	Snow.
● ♃ 26	29.858	38.6	29.796	40.5	36	37.8	39.2	30.3	39.2	0.128	E	A.M. Overcast. P.M. Fine.
♀ 27	29.779	41.9	29.720	42.2	38	38.4	40.4	36.2	40.4	0.014	N	A.M. Strong fog. P.M. Light deposition.
♄ 28	29.751	42.3	29.237	43.7	40	40.0	42.5	36.6	42.5		NNE	Foggy.
☉ 29	29.860	44.0	29.843	45.3	41	41.7	43.7	39.3	43.7		NNE	Foggy.
☽ 30	29.858	43.7	29.815	44.2	39	40.7	40.0	38.9	40.7		ESE	Foggy—light wind.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.994	44.7	29.935	46.7	38.9	40.7	44.3	36.7	44.9	0.693		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 29.966 | 3 P.M. 29.902 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge ..... = 83 feet 2½ in.

..... above the mean level of the Sea (presumed about) ..... = 95 feet.

The external Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ..... = 79 feet 0 in.

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The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR DECEMBER, 1829.

1829. Decem.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♂ 1	29.817	43.3	29.843	40.4	40	40.1	39.8	38.9	40.7		ESE	Light clouds and wind.
☿ 2	29.753	42.7	29.714	44.2	40	40.6	43.4	38.3	43.4		E	Foggy.
♃ 3	29.799	43.8	29.995	44.5	41	41.5	42.8	39.8	42.8		ESE	Foggy—light wind.
♀ 4	29.801	44.7	29.842	46.0	44	43.6	44.6	40.4	44.6		ESE	Light fog.
♄ 5	30.372	46.6	30.349	48.2	45	45.5	48.5	42.7	48.5		ESE	Foggy.
☉ 6	30.322	47.3	30.490	46.3	42	42.0	39.7	41.3	42.0		ENE	Overcast—light wind.
☽ 7	30.374	39.7	30.373	40.1	29	32.2	35.2	28.4	35.4		NE	Foggy. Hoar frost A.M.
♂ 8	30.313	39.8	30.341	39.5	29	35.0	35.4	30.9	35.4		WSW	Overcast—light wind.
☿ 9	30.379	39.3	30.291	38.2	29	33.4	36.0	31.4	36.0		NNE	A.M. Foggy. P.M. Fine.
○ ♃ 10	30.139	38.2	30.100	38.6	33	35.7	36.2	32.3	36.2		ENE	Overcast.
♀ 11	30.163	40.0	30.151	41.0	31	37.6	39.2	31.7	39.2		SSE	A.M. Overcast. P.M. Fine.
♄ 12	30.180	40.6	30.172	42.0	35	38.7	41.2	35.0	42.6		SSW	Light clouds and wind.
☉ 13	30.230	43.2	30.213	46.4	39	39.7	46.0	37.7	46.0		S	Clear and cloudless—light wind.
☽ 14	30.304	43.3	30.306	43.8	37	37.0	39.6	35.2	39.6		N	Strong fog.
♂ 15	30.341	40.0	30.306	40.8	33	33.7	39.6	31.6	39.6		N	Fog.
☿ 16	30.268	41.3	30.191	41.2	37	37.0	39.5	32.7	39.5		N	Lightly cloudy. Evening, drizzling rain.
♃ 17	29.985	39.6	29.884	40.6	35	35.3	37.5	34.4	37.5	0.017	NNE	Overcast.
♀ 18	29.698	40.3	29.672	41.4	35	36.0	38.0	31.8	38.3		NE	Cloudy and foggy. A.M. light snow.
♄ 19	29.868	39.3	29.828	40.4	34	35.6	36.5	32.8	37.0		N	Light clouds and wind.
☉ 20	29.852	37.6	29.877	37.2	26	32.3	33.1	30.7	33.1		N	Overcast—light wind. A.M. Fall of snow, early.
☽ 21	29.918	34.7	29.830	36.0	25	28.3	33.0	26.3	33.0		WSW	Foggy. Snow P.M.
♂ 22	29.721	34.7	29.716	36.8	30	32.3	34.8	27.0	34.8	0.033	E	Overcast.
☿ 23	29.747	34.6	29.678	32.5	27	31.7	29.0	29.7	31.7	0.044	NNE	Overcast—brisk wind.
♃ 24	29.714	30.8	29.728	31.8	25	27.7	28.9	24.6	30.5	0.008	N	Overcast. Snow A.M.
♀ 25	30.096	32.9	30.067	33.8	25	29.4	30.6	26.5	30.6		NE	Light clouds and wind.
● ♄ 26	30.307	30.5	30.330	30.5	22	27.2	28.0	25.6	28.0		N	Fine—light clouds.
☉ 27	30.403	29.2	30.380	30.8	24	25.8	28.5	25.5	28.5		NE	A.M. Fine. P.M. Overcast.
☽ 28	30.329	26.5	30.230	28.6	20	20.5	29.8	18.2	29.8		NW	Overcast.
♂ 29	30.302	28.0	30.313	28.2	21	24.5	26.5	20.2	27.2		NE	Overcast—light fog.
☿ 30	30.413	28.8	30.419	30.2	20	27.5	30.0	23.8	30.0		S	Overcast—light wind.
♃ 31	30.500	30.5	30.539	32.4	25	31.0	31.8	27.5	31.8		E	A.M. Overcast. P.M. Fine.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	30.109	37.8	30.099	38.5	31.5	34.1	36.2	31.4	36.6	0.102		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. ..... { 9 A.M. 30.098 } { 3 P.M. 30.086 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge..... = 83 feet 2½ in.

.....above the mean level of the Sea (presumed about) ..... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House..... = 79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.